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PROCEEDINGS

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CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 27, 1843.

On the Foundation of Algebra, No. III. By Augustus De Morgan, of Trinity College, Professor of Mathematics in University College, London, &c.

In the second paper of this series a general definition of the operation A^B was laid down, A and B being each of them any form of $p+q\sqrt{-1}$. The logarithm (or as Mr. De Morgan calls it, the *logometer*) of a line is thus described :—a line whose projection on the unit-axis is the logarithm of the length, and whose projection on the perpendicular is the angle made with the unit-axis (or its arc to a radius unity). Thus a line r inclined at an angle θ has for its logometer a line $\sqrt{(\log^2 r+\theta^2)}$ inclined at an angle whose tangent is $\theta : \log r$. This being premised, the universal definition of A^B is the line whose logometer is $B \times$ logom. A.

The object of this third paper is to show that the preceding definition of the logometer is not the most general. Take any two lines whatsoever passing through the origin, and style them the bases of length and direction. Set off on the first a line representing the logarithm of the length in question, and on the second a line representing the angle it makes with the unit-axis, both on any scale of representation. Then the diagonal of the parallelogram described on the lines just set off is a logometer to the length and direction from which it was derived; and if under this meaning of the word logometer the preceding definition of A^B be employed, the equations

$$A^B A^C = A^{B+C}, \quad A^B C^B = (AC)^B, \quad (A^B)^C = A^{BC}$$

are universally true.

There is no necessity for the introduction of this more general system, since all its results can be expressed in terms of those of the more simple definition in the second paper. This new definition of the logometer is really nothing more than the process answering to the extension of the theory of logarithms from the system constructed on the Naperian base, to that which is on any base whatsoever.

On the Measure of the Force of Testimony in cases of Legal Evidence. By John Tozer, Esq., M.A., Barrister-at-Law, Fellow of Gonville and Caius College.

The object of this paper is to show that the assumptions made by some English legal authorities on this subject, in opposition to the principles established by scientific processes, are not justified.

The views more particularly dissented from, as extracted from a work of high legal authority, are thus enunciated :—

"The notions of those who have supposed that mere moral probabilities or relations could ever be represented by numbers or space, and thus be subjected to arithmetical analysis, cannot but be regarded as visionary and chimerical.

"Whenever the probability is of a definite and limited nature (whether in the proportion of one hundred to one or of one thousand to one, is immaterial), it cannot be safely made the ground of conviction ; for to act upon it in any case would be to decide, that for the sake of convicting many criminals the life of one innocent man might be sacrificed.

"The distinction between evidence of a conclusive tendency which is sufficient for the purpose, and that which is inconclusive, appears to be this : the latter is limited and concluded by some degree or other of finite probability beyond which it cannot go ; the former, though not demonstrative, is attended with a degree of probability of an indefinite and unlimited nature."

The method pursued is that of investigating algebraic expressions for the probabilities that the allegations made in a case which actually occurred, the trial of a female for murder, are true ; and thence deducing an expression for the probability of the truth of the charge, in passing from the symbolical to the numerical expression, the numbers employed are not the actual values of the symbols but their limiting values ; the resulting number is therefore a fraction which is not less than the value of the probability of the truth of the principal allegation, this being what in practice is required.

The conclusion arrived at is, that the mode of estimating the force of evidence employed in a court is a process which algebraic investigation analyses, and of which it explains the theory, and an approximation to a result which is obtained with accuracy by assigning numerical values to the algebraic symbols : a clear conception of the nature of the practical process, it is conceived, must render its application more accurate, and to the extent of affording this the investigation is deemed to be of practical utility.

December 11, 1843.

On the Motion of Glaciers. By William Hopkins, M.A., F.R.S., Fellow of the Society (Second Memoir).

In a previous memoir Mr. Hopkins had given the details of certain experiments, by which it was proved that ice will descend with

a very slow unaccelerated motion down an inclined plane, presenting a surface like that of a common slab of paving-stone, at an angle scarcely exceeding half a degree (and probably also at still smaller angles), provided the lower surface of the ice in immediate contact with the inclined plane be in a state of constant but slow disintegration. This experimental conclusion was brought forward in support of the *sliding theory* of De Saussure, and the author endeavoured to explain, according to that theory, different phænomena connected with the motion of glaciers. He there considered glacial ice as a *solid* substance, having a certain degree of *plasticity* and *flexibility*, and the general mass of the glacier as a *dislocated* mass, the greater motion of the central portion of the glacier being much facilitated by these dislocations, though due partly, but in a comparatively small degree, to the plasticity of the general mass. In the present memoir Mr. Hopkins considers what would be the nature of the motion under other hypotheses respecting the constitution of glaciers. (1.) The lower part of a glacier may be conceived to be crushed, and consequently disintegrated, by the superincumbent weight, each component particle still retaining its solidity; or (2.) the whole mass may be conceived to be *plastic*, and to move by a change of form, produced by gravity, in each component element. The author contends, if either of these hypotheses were true, that *cæteris paribus*, the more superficial portion of the mass must tend to move the faster as the depth of the glacier should be greater; and that, consequently, the part of the glacier near the upper extremity must generally tend to move much faster than that near the lower extremity, assuming always the whole, or much the greater part of glacial motion, to be due to the plasticity of the mass, and to be independent of *sliding* over its bed. But in such case it is manifest that the general state of a glacier must be one of *longitudinal compression*, more particularly during the summer months, when the motion is greatest. Now the author contends that the general existence of transverse fissures (at least during summer) is a conclusive proof against the existence of general longitudinal compression; and he observes that no observer ventured to assert the fact of such compression to be deducible from actual observation. He conceives this to be a serious objection to the hypothesis here considered.

In this memoir Mr. Hopkins has also investigated the directions in which transverse fissures must be formed when referrible to the internal tensions superinduced by the conditions to which glaciers in general are subjected, and more especially by the more rapid motion of their central portions.

Assuming the velocity of each particle of the glacier to be the same in any vertical line (which is at least true at points not remote from the surface), the glacier may, in this investigation, be considered as a *lamina*. In this lamina take a rectangular element having two of its sides parallel to the axis of the glacier, and, therefore, the remaining sides perpendicular to it. Let X denote the intensity of the force acting *normally* to these latter sides of the element, Y that of the force acting normally to the two former sides. Also let f de-

note the intensity of the force acting *tangentially* on the sides on which X acts normally. It is proved that f will also be the intensity of the tangential force on the other two sides. Then, if θ be the angle which the line of maximum tension through the proposed element makes with the axis of the glacier, it is proved that

$$\tan 2\theta = \frac{2f}{X - Y},$$

where X and Y are *tensions*. If either be a *pressure*, it must be made negative.

If the maximum tension become greater than the cohesion of the ice, a fissure will be formed in a direction perpendicular to that of the tension at each point, or at least approximately so. Consequently, the line whose direction is defined by the angle θ, will be a normal to the curve of fracture. Now, taking the case in which the glacial valley contracts in descending (which is the more frequent case), Y is doubtless most frequently a *pressure*, in which case

$$\tan 2\theta = \frac{2f}{X + Y};$$

also f will be greatest at the sides (where the velocities of particles in a transverse line vary most rapidly), and will vanish at the centre. Hence θ will vanish at the centre of the glacier, and will increase towards the sides, since the change in the value of the denominator cannot be great. Consequently, if a fissure were continued across the glacier it would form a curve, meeting the axis of the glacier at right angles; and its convexity will be turned towards the upper extremity of the glacier, for the line defined by the angle θ, or the normal to the curve, meets the axis of the glacier when produced towards its *lower* extremity. This is the well-known character of transverse fissures, which the author conceives to be thus completely accounted for. In the previous memoir above referred to, this curious character had been very imperfectly explained by referring it to the action of the longitudinal tension (X) alone.

In conclusion the author has replied to the objections against the *sliding theory* urged by Prof. Forbes and others.

February 5, 1844.

On the Fundamental Antithesis of Philosophy. By W. Whewell, D.D.

The fundamental antithesis here spoken of, is that which is variously expressed by the opposition of thoughts and things, theory and fact, ideas and senses, necessary and experimental truth; also by the opposition of reflection and sensation, subject and object. It is remarked that we can have no knowledge without the union, no philosophy without the separation of these two elements. This fundamental antithesis of philosophy is an antithesis of inseparable ele-

ments. It is also shown that the terms which denote the two elements of this antithesis cannot in any case be applied absolutely and exclusively. We cannot say, this is a fact and not a theory, or this is a theory and not a fact; for a true theory is a fact; a fact is a familiar theory. It was further observed, that the antithesis being inseparable, one element seems, and is asserted, in different systems of philosophy, to be derived from the other; ideas from experience, or experience from ideas. But we must always have both elements: thus in mechanics, and in our experience, we have necessary principles, such as that every event must have a cause; and in chemistry also other necessary principles, as that the chemical composition of a body determines its kind and properties.

March 4, 1844.

On the Method of Least Squares. By R. L. Ellis, Esq.

The aim of this paper is to give a succinct exposition of the different demonstrations by which it has been proposed to establish the validity of the rule known as the method of least squares. The first demonstration of this celebrated rule (which had been previously proposed by Legendre) is that given by Gauss in the *Theoria Motûs*. The next appears to be that of Laplace, which has been followed, without variation of principle, by Poisson and other French writers. The demonstration of Gauss is based upon the assumption, that the arithmetical mean is the most probable result to be derived from a series of direct observations of an unknown magnitude. This assumption is alleged by Laplace to be altogether precarious; and it appears that Gauss acquiesced in this remark, as he subsequently, in the *Theoria Combinationis Observationum*, produced another demonstration, which is independent of this assumption. As the first method of Gauss has been followed by later writers, of whom Encke is one, it seemed desirable to endeavour to ascertain if the objection of Laplace be well-founded; and this the writer has attempted to do in the first part of the present communication. His conclusion is, that although the practice of adopting the arithmetical mean as an approximation to the true value of the unknown magnitude observed, is founded on just principles, yet that we are not entitled to say that it leads to the most *probable* result; and consequently that the demonstration in question is invalid.

The writer then proceeds to consider Laplace's demonstration. This involves no precarious assumption, but the mathematical part of the investigation is of very considerable difficulty, and cannot be said to be altogether free from doubt. For Laplace's analysis, another, founded on a theorem which was first made use of by Fourier in his researches on heat, is substituted; and by this change the mathematical difficulties of the subject are very much diminished. An attempt is also made to test the accuracy of Laplace's methods by reference to a particular case.

The third part of the paper relates to Gauss's second method. The relation in which it stands to that of Laplace is distinctly pointed out; the difference between them arising merely from this, that whereas Laplace considered the importance of a positive or negative error (that is, of an error in excess or defect) to be proportional to its arithmetical magnitude, Gauss assumes the square of the magnitude of the error as the measure of its importance, alleging that this importance not being a magnitude, does not strictly admit of numerical evaluation; that some assumption is therefore requisite, and that that which he proposes is not more arbitrary than Laplace's, while, from the absence of discontinuity, it leads to far simpler and more satisfactory calculations.

The writer then shows that neither Laplace's investigations, nor that of Gauss in the *Theoria Combinationis Observationum*, tends to prove, that the results of the method of least squares are the most *probable* of all possible results. This point, with regard to which there has occasionally been some degree of confusion, seems to be essential to a just apprehension of the nature of the subject. It may be remarked, with reference to it, that Laplace uniformly speaks of the method of least squares as the most *advantageous* method of combining discordant observations, or as that which gives the most *advantageous* results, and never as a method by which the most probable results are to be obtained.

Lastly, the writer proceeds to consider three demonstrations of the method of least squares, given by Mr. Ivory in the Philosophical Magazine. These demonstrations are independent of the theory of probabilities. The first is founded upon an assumed analogy between the equilibrium of weights on a lever and the combination of discordant observations; the second upon another unsatisfactory analogy; and the third upon the principle that the error committed at one observation is independent of that committed at any other. None of these demonstrations appear to the writer to be at all conclusive, but they seemed to deserve consideration, not only from the high reputation of their author, but also from the terms in which they have been mentioned in a recent work on the theory of probabilities.

On Divergent Series, and various Points of Analysis connected with them. By Augustus De Morgan, Esq.

The author states that he does not pretend to have perfect confidence even in convergent series. It is the main object of his paper to show that the continental analysts are not justified in their rejection of some classes of divergency, and retention of others, by anything but experience; that they have underrated the character of most which they reject, and overrated that of all they receive.

Divergent series are either of *infinite* divergence, such as $1 \pm 2 + 3 \pm 4 +$ &c., in which summation of terms may give any sum, however great; or of *finite* divergence, such as $\cos\theta + \cos 2\theta + \ldots$, in which no number of terms can give more than a certain quantity. The former are rejected by most modern continental writers, the latter are retained.

Section 1.—All divergent series, whether their divergence be finite or infinite, stand upon the same basis, and ought to be accepted or rejected together, as far as any grounds of confidence are concerned which are not directly derived from experience. The author examines the reasons which Poisson gives for maintaining the contradictory of the preceding. That great analyst considers $1-1+1-1+\ldots$ for instance, as $1-g+g^2-g^3+\ldots$, where g is less than unity by an infinitely small quantity. Mr. De Morgan maintains, that this method, if allowed in transformation of a finite diverging series into a convergent one, of which the convergency only begins after an infinite number of terms, must also be allowed, unless reason can be shown against it, in the destruction of the infinite character of an infinitely diverging series, by the tacit retention of the infinite remainder after an infinite number of terms.

The author would not use any series, so as to place absolute dependence upon their results, unless the producing functions were known: and this because series themselves neither show discontinuity nor infinity, when it takes place; and because it happens that divergent series, at least, and perhaps others, may represent one thing or another, according to the general form of which they are made particular cases.

Mr. De Morgan observes that a divergent series, which is not considered as arithmetically infinite, such as $1+2+4+\ldots$ may be so in reality, in particular cases. This series being called S, satisfies the equation $S=1+2S$, and this gives $S=-1$, the usual value of the series. But it is to be remembered that an equation may be a degenerate case of an equation of higher degree, in which case it has one or more roots infinite. An instance is produced in which $1+2+4+\ldots$ certainly represents infinity.

Finally, the author remarks that there is much more safety in series with terms alternately positive and negative, whether their divergence be finite or infinite, than in series of finite divergence, as such.

Section 2.—The operation of integration, as at present understood, is one of arithmetic, as distinguished from algebra, and must not be applied unreservedly to divergent series. The author supports the first part of this assertion upon the circumstance that the only definition of integration which is generally applicable is the *summatory* one, in which $\int \phi x \, dx$ does not mean the function whose differential coefficient is ϕx, but the limit of the summation expressed by $\Sigma(\phi x \Delta x)$. He then goes on to show instances in which it is unquestionably not allowable to apply integration to infinitely divergent series: and he asserts throughout the paper generally, that all the instances in which error has been shown to arise from the use of infinitely divergent series, have been those in which integration has been applied, and those only.

In this section warning is also given against the supposition that $0+0+0+\ldots$ must represent 0 in all cases.

Section 3.—It generally happens that the real analytical equivalent of the different values of an indeterminate expression, is the mean

of these different values. This principle is the one which was adopted by Leibnitz in his well-known explanation of the meaning of $1 - 1 + 1 - 1 + \ldots$ Without assuming that anything like proof can be given, the author notes, as instances in which the thing asserted is true, algebraical series, trigonometrical series, Fourier's integral, Poisson's expression of a function between any limits by an infinite series of trigonometrical integrals, and also the sine and cosine of *infinity*. Assuming $\int_a^b \phi x \, d x : (b - a)$ to represent the mean value of ϕx between a and b, the author tries what ought to be the value of $\tan \infty$, if this principle be true, and finds $\pm \sqrt{-1}$, which on trial is found to satisfy the fundamental equations of trigonometry.

Section 4.—Series of alternately positive and negative signs stand upon a much safer basis than those in which all the terms have the same signs, and that whether their divergence be finite or infinite.

It has long been observed, that when the terms of an alternating series begin by diminishing, even though they afterwards increase, the converging portion may be made effective in approximating to the arithmetical equivalent of the series. The error committed by stopping at any term is not so great as the first of the rejected terms. In many alternating series this has been proved to be true, and it seems never to have been supposed that the theorem was anything but universal. In this section instances are produced in which the theorem is not true; and at the same time various proofs of it are given, each of which applies to very extensive cases, and the tendency of which is to show that it is only under definite and unusual conditions that the theorem can fail. Still, however, no positive criterion is established for ascertaining whether the theorem be true or not in any particular case.

Section 5.—On double infinite series, in which the terms are infinitely continued in both directions.

It seems, in many different ways, that the series

$$\ldots + \phi(x-2) + \phi(x-1) + \phi x + \phi(x+1) + \phi(x+2) + \ldots$$

can be resolved, by analytical transformation, into $0 + 0 + 0 + 0 + \ldots$ When there is no discontinuity whatever in the relation between $\phi x + \phi(x+1) + \ldots$ the value of the preceding is 0. But when discontinuity does exist, the value of the series may be some other solution of $\psi(x+1) = \psi x$. This assertion, derived from observation of instances, is here discussed in the case of

$$\phi x = \frac{1}{1 + (b + c x)^2};$$

the value of the double series is obtained, and some corresponding products of an infinite number of factors are deduced.

April 20, 1844.

On the Transport of Erratic Blocks. By W. Hopkins, M.A., F.R.S. &c.

The principal object of this paper is to investigate the transporting power of currents of water in general, and to explain in particular the nature of those which would arise from the instantaneous or paroxysmal elevation of any considerable extent of the earth's surface lying beneath the surface of the sea. The author has termed them *elevation currents*. The immediate effect of an elevation like that just supposed, would be the elevation to a nearly equal height, of the surface of the superincumbent water, whence a great wave would diverge in all directions. Such a wave would be attended by a *current* in the direction of the wave's propagation, and has thence been called a *wave of translation*. When such a wave proceeds along a uniform canal, Mr. Russell has established experimentally the following facts:—

1. Every particle in the same transverse section of the canal has the same motion.

2. The velocity with which the wave is propagated is equal to that due to half the height of the crest of the wave above the bottom of the canal.

From these data the author has calculated the velocity of the currents which would necessarily attend these waves of elevation. It depends principally on the height of the elevation and the depth of the sea, while the time during which the current will flow depends principally on the extent of the elevated area and the depth of the sea. Thus if the depth of the sea should be 300 feet, and the height of the crest of the wave above the even surface of the sea (which may be considered as approximately the same as the elevation of the suddenly raised area) should be 50 feet, the wave would be propagated with a velocity of upwards of 70 miles an hour, and the attendant current would be upwards of 10 miles an hour. Also, if the elevated area were circular, the width of the wave would exceed the radius of the circle. The wave would have the essential character of a tidal wave termed a *bore*, except that it diverges in all directions, instead of proceeding along a confined channel.

The author next proceeds to calculate the motive power of currents of water. Let v be the velocity of the current, ρ_1 the density of the water, and S the area of a plane surface on which the current acts, and so placed as to make an angle θ with the direction of the current; then if R denote the whole normal action of the current on S, we have

$$R = \frac{v^2}{2} \rho_1 S \sin^2 \theta,$$

provided θ do not deviate too much from 90°. When $\theta = 90°$ the truth of this formula has been proved by numerous experiments, for all velocities up to 11 or 12 miles an hour, and may be assumed to

hold, at least approximately, for still greater velocities. It has also been proved experimentally to be approximately true for any value of θ not differing by more than 45° from a right angle, as is the case in the applications made of the formula.

The velocity of the current just sufficient to move a block will depend on the volume, the specific gravity, and the form of the block. If the block *slide*, much will depend on the nature of the surface over which it is transported, and thus a very uncertain element will be introduced into the calculations. This uncertainty, however, will be in a great degree removed if we calculate the force sufficient to make the block *roll*. Each block would present a separate problem if it were required to find accurately the current necessary to move it, but as great accuracy is not necessary in the cases here contemplated, it is sufficient to make the calculations for a few determinate and simple forms as those to which more irregular forms may be referred with a sufficient approximation to accuracy. Thus the author has considered the cases of blocks whose sections perpendicular to their length are squares, pentagons, hexagons, &c., and has calculated their dimensions, that a current of about 10 miles an hour might just be sufficient to make them move by rolling. Assuming the specific gravity of the blocks to be 2·5, we have the following results:—

1. A *parallelopiped.*

Side of the square section perpendicular to its length = 2·73 feet.

2. A *pentagonal prism.*

Side of the pentagonal section perpendicular to its length = 2·27 feet.

3. A *hexagonal prism.*

Side of hexagonal section perpendicular to its length = 2·3 feet.

When the motion takes place, as here supposed, in a direction perpendicular to the length of the block, the efficiency of the current to move it will evidently be independent of the length of the block. If we suppose the length of the parallelopiped to be equal to the side of a section of it taken as above, it becomes a cube; and if we take the lengths of the blocks in the other two cases to be equal to twice the length of the sides of their sections respectively, their lengths will not much exceed their heights. Then the weights of the blocks will be $1\frac{1}{2}$ ton in the first, nearly 3 tons in the second, and upwards of 4 tons in the third case. Again, if the block be an oblate spheroid resting with its axis vertical, and the polar axis $= \frac{3}{4}$ths of the equatorial diameter, the current of about 10 miles an hour will just make it roll if its height be about 2 feet, and its weight about 4 tons. If the polar axis $= \frac{5}{6}$ths of the equatorial diameter, the block will be just moved, provided its height be $3\frac{1}{3}$ feet and its weight 14 or 15 tons.

In this part of the investigation it is shown that the power of rapid currents to transport blocks of enormous magnitude is perfectly consistent with the almost inappreciable power of currents of which the velocity does not exceed, for instance, 2 miles an hour; for it is shown that *the weight of a block of given form and specific*

ravity, which may thus be moved, varies as the 6th power of the velo-ity of the current. Thus if a current of 10 miles an hour will just nove a block of a certain form, whose weight is 5 tons, a current of 5 miles an hour would move a block of similar form of upwards of 5 tons. A current of 20 miles an hour would, according to the ame law, move a block of 320 tons, while a current of 2 miles an iour would scarcely move a small pebble.

In the previous calculations the relation between the magnitude of he block and the velocity of the current has been determined on the upposition that the current, at the instant it acquires its greatest elocity, shall just be able to move the block, which would again be eft at rest without being moved through any sensible space. If the elocity be greater or the mass smaller, the block will be transported o a distance which the author has calculated. Let

v_2 be the velocity of a current just sufficient to move an assigned block;

v_1 the velocity of the transporting current acting on the above block, v_1 being greater than v_2;

the breadth of the great wave of translation producing the current;

h the height of the highest point of the wave above the level of the ocean;

H the depth of the ocean;

the space through which the block is transported by the wave.

The following Table gives corresponding values of these quanti-

H.	h.	V.	v_1.	v_2.	s.	s_0.
feet.	feet.	miles.	miles.	miles.		
200	50	62	12	5	$\frac{l}{30}$ nearly	$\frac{l}{10}$ nearly
				10	$\frac{l}{372}$ ...	
300	50	73	10·4	5	$\frac{l}{52}$ ...	$\frac{l}{14}$ ...
300	100	77	19·4	5	$\frac{l}{14}$ ...	$\frac{l}{8}$ ...
				10	$\frac{l}{34}$ ...	
400	100	86	17	5	$\frac{l}{20}$ ...	$\frac{l}{10}$ ...
				10	$\frac{l}{60}$ ...	
450	150	95	23·5	10	$\frac{l}{24}$ ...	$\frac{l}{8}$ ...
600	150	106	20·5	10	$\frac{l}{39}$ ...	$\frac{l}{10}$ ...
800	200	140	28	10	$\frac{l}{25}$ ...	$\frac{l}{10}$ ...

ties. The last column gives the corresponding value of the space (s_0) through which a particle of the water, or any body floating in the water, will be carried by the wave. The expressions for s and s_0 are

$$s = \frac{1}{2} \cdot \frac{(v_1 - v_2)^2}{V\ v_1} l,$$

$$s_0 = \frac{1}{2} \cdot \frac{v_1}{V} \cdot l,$$

V being much greater than v_1. [See preceding page.]

In estimating the magnitude of a block which may be moved by a given current, the transport is supposed to take place over a horizontal surface sufficiently hard and even for the block to roll upon it without impediment. In other states of the surface the transport might be more or less impeded. The constant action of denuding causes would be highly favourable to the transport by the successive removal of local impediments. The author conceives that the objection to this mode of transport, founded on inequalities of surface which now exist between the original site of a block and its present position, have been far too much insisted on by some geologists, for, he contends, such inequalities could not generally exist under the continued action of denuding causes, among the most powerful of which may be reckoned the transporting currents themselves.

It should be remarked, that it appears from the values of s given in the preceding table, that the space through which any considerable block could be moved by a single wave of elevation, is only equal to a small fraction of the breadth of the wave. Consequently, if such a block has been moved by this agency to a considerable distance from its original site, the transport must have been effected by a repetition of transporting waves; and, therefore, since a wave of considerable height can only be produced by a *sudden* elevation, this theory of transport is ultimately associated with the theory which attributes the more marked phænomena of geological elevation to a repetition of *paroxysmal* movements.

The author concludes with some general observations on the evidence by which we may hope to distinguish between the effects of the three different agencies to which the transport of blocks may be attributed—glaciers, floating ice, and currents of water. Large angular blocks in the immediate neighbourhood of glacial mountains (such as the alpine blocks) may doubtless, in many cases, be referred to glaciers, while the transport of similar blocks to great distances may be referred to floating ice. Smooth rounded blocks of smaller dimensions, especially when spread out with other detrital matter in layers of considerable horizontal extent, the author would refer to the action of aqueous currents.

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October 28, 1844.

On the Foundation of Algebra, No. IV.—On Triple Algebra. By Augustus De Morgan, Esq., of Trinity College.

The extensions which have successively been made in algebraical interpretation have been consequences of efforts to interpret symbols which *presented themselves* as necessary parts of the algebraical language which is suggested by arithmetic. The now well-known signification of $a+b\sqrt{-1}$ did not yield any new imaginary or unexplained quantities: and accordingly no effort (within the author's knowledge) was made to produce an algebra which should require three dimensions of space for its interpretation, until Sir William Rowan Hamilton wrote a paper (the first part of which appeared in the Philosophical Magazine * before the present one was begun) on a System of Quaternions. This system, as the name imports, involves four distinct species of units, one of which may by analogy be called *real*, the three others being *imaginaries*, as distinct from one another as the imaginary of ordinary algebra is from the real. These imaginaries are not deductions, but inventions; *their laws of action on each other are assigned*: this idea Mr. De Morgan desires to acknowledge as entirely borrowed from Sir William Hamilton.

Sir William Hamilton has rejected the idea of producing a triple algebra, apparently on account of the impossibility of forming one in which such a symbol as $a\xi+b\eta+c\zeta$ represents a line of the length $\sqrt{(a^2+b^2+c^2)}$. Mr. De Morgan does not admit the necessity of having a symmetrical function of a, b, c, and, throwing away this stipulation, points out a variety of triple systems, partially or wholly interpreted.

Sir William Hamilton's quaternion algebra is not entirely the same in its symbolical rules as the ordinary algebra: differing in that the equation $AB=BA$ is discarded and $AB=-BA$ supplies its place. Those of Mr. De Morgan's system, which are imperfect, all give $AB=BA$, but none of them give $A(BC)=(AB)C$, except in particular cases.

* Vol. xxv. pp. 10, 241.

Mr. De Morgan gives systems of triple algebra, which he distinguishes into quadratic, cubic, and biquadratic, according as the invented imaginary units represent square roots, cube roots, or square and fourth roots, of the negative real unit. It would not be easy in an abstract to give any account of these, but among them are found,—

1. An imperfect quadratic system, strongly resembling the common double algebra, and which would, but for its imperfect character, be at once recognised as the proper and natural extension of the interpretation of imaginary quantities to three dimensions of space: the ultimate symbol for a line is $l(\cos\theta + \sin\theta\sqrt{\ }-1)$.

2. An imperfect quadratic system, very like the former one, except in having a peculiar inversion in the operation of multiplication, and a somewhat remarkable mode of representing what would by analogy be called arithmetical multipliers.

3. A perfect quadratic system, the interpretation of which has considerable resemblance to that of the first-mentioned system, and is completely attainable, though not of great interest.

4. Three perfect cubic systems, each irreconcileable with the others, though closely connected with them. Each system presents a triple trigonometry, the cosine and *two sines* of which are each a function of two angles; but these can be easily expressed as functions of common circular and hyperbolic sines and cosines. The interpretations of these systems are very imperfect, and appear to present great difficulty, but their symbolical character is unimpeachable.

5. A perfect biquadratic system, which is of a redundant character, that is, its fundamental form represents a line drawn in space from a given origin, with a symbol to spare, which may represent the time of drawing it, its density, its tendency to a given position, &c. at pleasure. Many interpretations are attainable, but Mr. De Morgan does not pretend to say that he knows the one which ought to be adopted. It is singular that every attempt to reduce this algebra, by assigning a condition among the subsidiary symbols of its fundamental form, leads to an imperfect algebra. The system first mentioned in this abstract is one such result, and fails in its rules of multiplication, as before mentioned. Another is obtained, which is perfect as to its rules of multiplication, &c., but fails in its rules of addition.

Mr. De Morgan concludes by giving some formulæ which may be useful to those who would try to interpret algebra of three dimensions by the use of solid angles in the place of plane ones.

December 9, 1844.

On the Values of the Sine and Cosine of an Infinite Angle. By the Rev. S. Earnshaw, of St. John's College.

It has been usual with mathematicians to write zero as an equivalent for both $\sin x$ and $\cos x$ when x becomes infinite. The object

of this paper is to examine into the propriety of this usage. The inquiry derives importance from its bearing on the general correctness of Fourier's theorem for the transformation of functions, and from its affecting the truth of many remarkable results in definite integrals. Certain principles also which have been assumed and acted on by Poisson, Fourier, Cauchy and others, in treating of periodic infinite series, are examined, and shown to be untenable: for example, it is shown, that as $1-x$ approaches zero, $1-x+x^2-x^3+\ldots$ ad inf. does not approach $1-1+1-1+\ldots$ ad inf. as its limit; that this last series has not a unique value, and that its value is not $\frac{1}{2}$, as has generally been argued. It is also remarked that every series of the form $a_1x^\alpha+a_2x^\beta+\ldots a_nx^\nu+\ldots$ is discontinuous in those terms which are at an infinite distance from the first, unless the coefficients tend to zero as n and ν tend to ∞. The truth of this depends on a circumstance which does not seem to have been remarked before, viz. that however small $1-x$ may be, a value of ν can always be found so large that $(1-x)\nu$ may be finite, and therefore x^ν, which is equal to $(1-\overline{1-x})^\nu$, is not equal to 1 in the limit, but to $e^{\text{lim. of } (1-x)^\nu}$.

It is lastly proved that $\sin\infty$ and $\cos\infty$ are not equivalent to zero, whether we regard them as the results of integration between limits, or as the limiting cases of more general forms.

February 10, 1845.

On the Connexion between the Sciences of Mechanics and Geometry. By the Rev. H. Goodwin, of Caius College.

This paper contains an attempt to determine the ground of the truth of the elementary propositions of mechanics. The remarkable analogy between mechanics and geometry suggests the thought, that perhaps there may be something more than analogy, that in fact the basis of the two may be the same. The author endeavours to show that this is really the case; the ground of the reasoning is, that force is a *physical* expression of the two ideas of *magnitude* and *direction*, of which a straight line is the *geometrical* expression, and therefore that propositions which are true for one event are true for the other. Hence it is argued, that inasmuch as the giving two sides of a triangle gives the third, so that the third may be considered as the *resultant* of the two already given, so if the two sides represent *forces*, the third will still represent the *resultant* of the two forces already given.

Reasoning of this kind does not, of course, admit of a very demonstrative character *primâ facie*; it is the author's design rather to point out a path to the truth, than to assert that he has cleared away every difficulty.

The subject is further elucidated by the application of the remarkable symbol $e^{\theta\sqrt{-1}}$, a symbol which in geometry serves to indicate the direction in which a line is drawn with respect to a given fixed line; the same symbol is perfectly applicable as a sign of affection for forces, and hence the conclusion is strengthened that the ground of truth in the two sciences is the same.

The reasoning of this paper extends not only to forces, but also to velocities and moments, and to all expressions of whatever kind of the pure ideas of *magnitude* and *direction*.

If the author's reasoning be sound, the elementary propositions of mechanics are *necessary truths* in as strict, in fact, in exactly the same, sense as the elementary propositions of geometry; and to a mind which dwells upon them, the truths of the one science ought to appear in as axiomatic a light as those of the other.

April 14, 1845.

On the Theories of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids. By G. G. Stokes, M.A., Fellow of Pembroke College.

The theory of the equilibrium of fluids depends on the fundamental principle, that the mutual action of two contiguous portions of a fluid is normal to the surface which separates them. This principle is assumed to be true in the common theory of fluid motion. But although the theory of hydrostatics is fully borne out by experiment, there are many instances of fluid motion, the laws of which entirely depend on a certain tangential force called into play by the sliding of one portion of fluid over another, or over the surface of a solid. The object of the first part of this paper is to form the equations of motion of a fluid when account is taken of this tangential force, and consequently the pressure not supposed normal to the surface on which it acts, nor alike in all directions.

Since the pressure in a fluid, or medium of any sort, arises directly from molecular action, being in fact merely a quantity by the introduction of which we may dispense with the more immediate consideration of the molecular forces, and since the molecular forces are sensible at only insensible distances, it follows that the pressure at any point depends only on the state of the fluid in the immediate neighbourhood of that point. Let the system of pressures which exists about any point P of a fluid in motion be decomposed into a normal pressure p, alike in all directions, due to the degree of compression of the fluid about P, and a system S of pressures due to the motion. The author assumes that the pressures belonging to the system S depend only on the relative velocities of the parts of the fluid immediately about P, as expressed by the nine differential coefficients of u, v and w with respect to x, y and z. [The common notation is here employed.] He assumes, further, that the relative velocities due to any arbitrary motion of rotation may be eliminated

without affecting the pressures of the system S. Choosing for the motion of rotation that for which the angular velocities are $\frac{1}{2}\left(\frac{dw}{dy} - \frac{dv}{dz}\right)$ about the axis of x, with similar expressions for the axes of y and z, the residual relative motion depends on only six independent quantities. Considering only this residual relative motion, the author shows that there are always three directions, which he names *axes of extension*, at right angles to one another, such that if they be made the axes of x_1, y_1, z_1, the resolved parts of the relative velocity of the point P′, whose relative co-ordinates are x_1, y_1, z_1, will be $e' x_1$, $e'' y_1$, $e''' z_1$ along the three axes of extension respectively, the point P′ being supposed indefinitely near to P. Thus the system of pressures S is made to depend on the three quantities e', e'', e''', which in the case of an incompressible fluid are connected by the equation $e' + e'' + e''' = 0$. Moreover, on account of the symmetry of the motion, the pressures on planes perpendicular to the axes of extension will be normal to those planes. They will here be denoted by p', p'', p'''.

By what precedes, any one of these pressures, as p', will be expressed by $\phi(e', e'', e''')$, the function ϕ being symmetrical with respect to the second and third variables. For reasons stated in the paper itself, the author was led to take, as the form of the function ϕ, $\mathfrak{C} e' + \mathfrak{C}'(e'' + e''')$. The general expressions for the pressures would thus contain two arbitrary constants (or rather functions of the pressure and temperature), which in the case of an incompressible fluid would unite into one. But it is shown by the author, that in all probability $p' = 0$ when $e' = e'' = e'''$; and he accordingly makes this assumption, which reduces the two constants to one, even in the case of a gas. The expression for p' finally adopted is $\frac{2}{3}\mu(e'' = e''' - 2e')$.

The pressures on three planes passing through P being known, the pressure on any other plane passing through that point may be found by the consideration of the motion of an indefinitely small tetrahedron of the fluid. Thus expressions are obtained for the pressures on planes parallel to the co-ordinate planes. These expressions, however, contain quantities which refer to the axes of extension; and it is necessary to transform them into others containing quantities which refer to the axes of co-ordinates. This transformation is easily effected by means of an artifice, and then no difficulty remains in forming the equations of motion. When μ is supposed to be constant, a supposition which it is shown may in many cases be made, the equations thus obtained are those which would be obtained from the common equations by subtracting

$$\mu\left(\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2}\right) + \frac{\mu}{3}\frac{d}{dx}\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right)$$

from $\frac{dp}{dx}$ in the first, and making similar changes in the other two.

The particular conditions which must be satisfied at the boundaries

of the fluid are then considered, and the general equations applied to a few simple cases.

On considering these equations the author was led to observe, that both Lagrange's and Poisson's proofs of the theorem that $u\,dx + v\,dy + w\,dz$ is always an exact differential when it is so at any instant (the pressure being supposed equal in all directions), would still apply, whereas the theorem is manifestly untrue when the tangential force is taken into account. This led him to perceive that one objection to these proofs is of essential importance. He has given a new proof of the theorem, which however was not necessary to establish it, as it has been proved by M. Cauchy in a manner perfectly satisfactory.

The methods employed in this paper in the case of fluids apply with equal facility to the determination of the equations of equilibrium and motion of homogeneous, uncrystallized, elastic solids, the only difference being that we have to deal with relative velocities in the former case, and with relative displacements in the latter. The only assumption which it is necessary to make, is that the pressures are linear functions of the displacements, or rather relative displacements, the displacements being throughout supposed extremely small. The equations thus arrived at contain two arbitrary constants, and agree with those obtained in a different manner by M. Cauchy. If we suppose a certain relation to hold good between these constants, the equations reduce themselves to Poisson's, which contain but one arbitrary constant.

The equations of fluid motion which would have been arrived at by the method of this paper if the two constants $\mathfrak{E}$, $\mathfrak{E}'$ had been retained, have been already obtained by Poisson in a very different manner. The author has shown, that according to Poisson's own principles, a relation may be obtained between his two constants, which reduces his equations to those finally adopted in this paper.

There is one hypothesis made by Poisson in his theory of elastic solids, by virtue of which his equations contain but one arbitrary constant, which the author has pointed out reasons for regarding as improbable. He has also shown that there is ground to believe that the cubical compressibility of solids, as deduced by means of Poisson's theory from their extensibility when formed into rods or wires, is much too great, a conclusion which he afterwards found had been previously established by the experiments of Prof. Oersted.

The equations of motion of elastic solids with two arbitrary constants, are the same as those which have been obtained by different authors as the equations of motion of the luminiferous æther in vacuum. In the concluding part of his paper the author has endeavoured to show that it is probable, or at least quite conceivable, that the same equations should apply to the motion of a solid, and to those very small motions of a fluid, such as the æther, which according to the undulatory theory constitute light.

May 12, 1845.

On the Aberration of Light. By G. G. Stokes, M.A., Fellow of Pembroke College.

In the common explanation of aberration, it is supposed that light comes in a straight line from a heavenly body to the surface of the earth, except in so far as it is bent by refraction. This, of course, would follow at once from the theory of emissions; but it appears at first sight difficult to reconcile with the theory of undulations, unless we make the startling supposition that the æther passes freely through the earth as the earth moves round the sun. The object of this paper is to show that if we make the following suppositions, that the earth in its motion pushes the æther out of its way, that the æther close to the surface of the earth is at rest relatively to the earth, and that light is propagated through the disturbed æther as we suppose sound to be propagated air in motion, the observed law of aberration will still result, provided the motion of the æther be such that $u\,dx + v\,dy + w\,dz$ is an exact differential, where u, v, w are the resolved parts of the velocity of any particle of the æ her along the rectangular axes of x, y, z.

On the Pure Science of Magnitude and Direction. By the Rev. H. Goodwin, Fellow of Caius College, and of the Cambridge Philosophical Society.

This memoir may be considered in some degree supplementary to the preceding one by the same author, "On the Connexion of the Sciences of Mechanics and Geometry." In that memoir it was argued, that if the views there advanced were sound, there must be such a science as that of *pure direction*, or rather a *pure science of magnitude and direction* which should include within itself the sciences of geometry, of kinematics, and of mechanics; in this the attempt is made to establish mathematically the fundamental proposition of such a science.

By making use of De Moivre's formula, the author conceives himself to have established this proposition, that if P represents the magnitude of any cause which varies uniformly and continuously into its exact opposite, *i. e.* into $-$ P while its direction varies uniformly from a given direction to the exactly opposite direction; and if θ be the angle which the direction of P makes with a given direction, then P is equivalent to two causes, P $\cos\theta$ in that given direction, and P $\sin\theta$ in the direction perpendicular to it.

The author is aware of the improbability which may appear to exist, that so general a proposition should be susceptible of proof without reference to particular instances, and has therefore endeavoured to obviate some objections, which will be more or less strongly felt, according to the nature of the philosophy of knowledge adopted by the mind which makes them, and which in some cases will probably be invincible.

The memoir concludes with some remarks on the general question

of the transition of a quantity from the + to the — affection, which the author conceives to be illustrative of his general design.

December 8, 1845.

On the Heights of the Aurora Borealis of September 17 and October 12, 1833. By Professor Potter, A.M., of Queen's College.

The data for the calculations are almost entirely taken from the conspectuses of the observations printed and distributed in 1833 by the British Association; and although so long time has elapsed, no calculations of the heights of the phænomena, which are the first steps to be taken in finding the nature of the meteor, have, to the author's knowledge, been hitherto published; the only imperfect discussion being given in the Philosophical Magazine for December 1833.

The observers of the display of September 17, were Mr. J. Phillips, at York; Mr. Clare, Mr. Hadfield and the author, at or near Manchester; Professor Airy, at Cambridge; and the Hon. C. Harris, near Gosport.

The observers of that on October 12, were Professor Sedgwick, at Dent; Mr. W. L. Wharton, near Guisborough; Mr. J. Phillips, at York; Mr. Clare, Mr. Hadfield and the author, at or near Manchester; Dr. Robinson, at Armagh; Professor Airy, at Cambridge; and the Hon. C. Harris, at Heron Court.

The observations of the aurora of September 17 at Cambridge at $8^h\ 25^m$ Greenwich time, taken with those at Manchester at $8^h\ 24^m$, give the height of the lower edge of the arch 56 English miles, and of the upper edge 71 miles.

The observations of another arch, seen from $10^h\ 49^m$ to $11^h\ 19^m$ at York, and from $10^h\ 49\frac{1}{2}^m$ to $11^h\ 4\frac{1}{2}^m$ near Gosport, give the height of the lower edge 389 miles.

The observations on October 12, at $7^h\ 56^m$ at York and at $7^h\ 54^m$ at Cambridge, give the height of the upper edge of an arch 72·2 miles.

The observations at Guisborough at $8^h\ 20^m$, and at Heron Court at $8^h\ 22^m$, give the height of the under edge of the arch seen at that time 70·9 miles, and of the upper edge 85·5 miles.

The observations at Dent at $8^h\ 55^m$, taken with others at Manchester at $8^h\ 54^m$, give the height of the upper edge of that arch 84·97 miles.

The last arch remained stationary about a quarter of an hour, and therefore the observations are the more valuable; but combining an observation at Armagh with those at Manchester, the height comes out only 64·47 miles; and even with the utmost allowable latitude to the deductions from the observations, the height comes out 66·5 miles.

The last arch having been noticed to have risen to a higher alti-

tude at the same places, a calculation with the corresponding data gives the height 65·4 miles.

These last three results are remarkably in accordance with each other, but considerably different from those for other places at nearly the same time; so that probably the method which was used, of obtaining a base line by projecting the places of observation upon an intermediate magnetic meridian, is only approximately correct, from the course of the arch over the earth's surface, rather than geometrical reasons.

Another arch was noted by most of the observers from $10^h\ 34^m$ to $10^h\ 45^m$. The observations at Dent at $10^h\ 40^m$, and Heron Court at $10^h\ 37^m$, give the height of the upper edge 59·4 miles.

An observation made by the author on the extent of the arch, on September 17. upon the horizon at $8^h\ 40\frac{1}{2}^m$, and its altitude, for application to the formula he has given in the Edinburgh Journal of Science, before it was joined with the Philosophical Magazine, for determining the height from observations at one place by the help of an hypothesis, gave the height 53·9 miles, which is a near approximation to the height found by the trigonometrical method for $8^h\ 25^m$.

The author concludes that the meteor occurs immediately beyond the ordinary limits assigned to the earth's atmosphere, and from that to very much greater altitudes, as shown by many other calculations; and states his conviction that the meteor will be some time observed with much more accurate means than hitherto, from its connexion with the earth's magnetism.

February 23, 1846.

Analytical Investigation of the Disease prevalent in the Potato during the year 1845. By Geo. Kemp., M.D., Pet. Coll.

This communication may be resolved into two parts; the analysis of the diseased portion of the potato, as compared with Boussingault's analysis of the healthy tuber, and certain deductions derived from the empirical formulæ proposed as representing their respective compositions.

The author recognises three stages of the disease: the first appearing as dark brown patches under the skin; the second as striæ of the same colour proceeding towards the centre; and the third as a soft, pultaceous, blackish, and offensive mass, in which all traces of organization are lost.

From the impossibility of isolating the portions affected by the disease, in the first two stages, from the surrounding sound parts, the examination was principally directed to the third stage.

A potato having been selected in which the above characters were well-developed, a sufficiently large portion for comparison still remaining perfectly sound, gave the following results as indicative

of the relative proportions of organic and inorganic matter of the sound and unsound part.

Of the sound portion,—

I. 247 milligrammes gave 10·5 milligrammes of ash.
II. 205·5 ... ... 8·5 ... ...

Of the unsound portion,—

I. 311 milligrammes gave 18 milligrammes of ash.
II. 234 ... ... 13 ... ...
III. 236 ... ... 13·5 ... ..

Or, reducing to 100 parts, the sound portion consists of—

	I.	II.
Organic matter....	95·75	95·86
Inorganic matter ..	4·25	4·14
	100·00	100·00

whilst the unsound portion gives,—

	I.	II.	III.
Organic matter	94·22	94·45	94·29
Inorganic matter....	5·78	5·55	5·71
	100·00	100·00	100·00

The mean of the former is 4·19 per cent. of ash, that of the latter 5·68, making an excess of 1·49 per cent. arising from loss of organic matter.

The ultimate analysis of the unsound portion afforded the following results:—

I. 155 milligrammes gave carbonic acid 238·5, water 98.
II. 132 ... ... 202 ... 78.
III. 163 ... ... 251, water not estimated.

Mean of two analyses for nitrogen, after the method of MM. Varrentrapp and Will, 1·23 per cent.

These data furnish the following summary:—

	I.	II.	III.
Carbon	42·09	41·73	41·99
Hydrogen ..	7·02	6·56	7·02
Nitrogen....	1·23	1·23	1·23
Oxygen	43·98	44·80	44·08
Ash.....	5·68	5·68	5·68
	100·00	100·00	100·00

The analysis of the sound potato, by Boussingault, is as follows:—

Carbon	44·1
Hydrogen	5·8
Nitrogen	1·2
Oxygen	43·9
Ash	5·0
	100·0

The object of the second part of the communication is to show, that, while Boussingault's analysis of the sound potato may be expressed by an empirical formula, representing the elements of proteine, starch, and cellulose, the analysis of the tuber, after undergoing the action of the late prevalent disease, admits of no such solution; but may be expressed by an empirical formula, representing proteine, starch, and butyric acid, with a very large excess of the elements of water.

Butyric acid has been found in the diseased potato by Mr. Tilley; but the author's principal object is to connect the changes developed by his analyses with the researches of Erdmann, Marchand, and Scharling, on the germination of seeds and tubers. These researches are totally independent and irrespective of the disease in question, whilst it is clear that the same changes occur in both cases. After reviewing the physical circumstances with respect to soil and culture, which have proved remarkably favourable to the development of the morbid changes, the author arrives at the general conclusion, that the disease in question essentially consists in an unnatural tendency to premature germination.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

February 1846.

The Mathematical Theory of the two great Solitary Waves of the first Order. By S. Earnshaw, M.A.

The nomenclature of this paper is adopted from a Report on Waves by Mr. J. S. Russell, printed in the Proceedings of the British Association. From the extreme comprehensiveness of the equations of fluid motion, the author infers a necessity of appealing to experiments for the suggestion of data which may be used in modifying the generality of those equations so as to suit actual cases of known fluid motion. With this view he has made use of the experiments recorded in Mr. Russell's report, and thence selected the two following properties :—1st. The velocity of transmission of a wave in a uniform canal is constant. 2nd. The horizontal velocity is the same for all particles situated in a vertical plane, cutting the axis of the canal at right angles. By reference to Mr. Russell's report, it will be seen that these two properties, selected on account of their simplicity and ready experimental examination, are distinguishing characteristics of what he has denominated the two great solitary waves of the first order. By the aid of them the equations of motion take such modified forms as to admit of exact integration; so that without employing any analytical approximations the author is enabled to obtain theoretical expressions for all the circumstances of the two solitary waves. The results are tested by a comparison of the velocities of transmission of various waves given by theory and by experiment. The greatest difference of these in the case of the positive wave is not found to exceed $\frac{1}{38}$th part of the whole velocity; but in the case of the negative wave it is found to be much greater, and to amount in one instance to as much as $\frac{1}{6}$th of the whole velocity. The reason of this discrepancy is conjectured; and the agreement in the case of the positive wave is considered to be exact.

It is found in the course of the investigation that one of the necessary conditions of fluid motion is not satisfied; and it is shown that it cannot be satisfied as long as the two principles, adopted from Mr. Russell's report, are supposed to coexist. They are proved in fact to be incompatible with each other. But as the second prin-

ciple was found by Mr. Russell to be so nearly exact that he could not detect any deviation from it in his experiments, it is shown by theory that from this circumstance there will be a rapid degradation of the summit of the wave, and a consequent loss of the velocity of its transmission, both which results of theory were observed to be true experimentally. The memoir concludes with pointing out the agreement of theory with some minor phænomena noticed by Russell.

May 11, 1846.

A theory of Luminous Rays on the Hypothesis of Undulations. By the Rev. J. Challis, M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.

In this communication, the æther, which is supposed to be the medium of the transmission of light, is regarded as a continuous fluid substance, such that small increments of its pressure are proportional to small increments of density, and is treated mathematically according to hydrodynamical principles. The author shows, by means of the usual hydrodynamical equations, and by an additional equation of continuity, the existence and necessity of which he has considered in the Cambridge Philosophical Transactions (vol. vii. part iii. pp. 385 and 386), that a given slender cylindrical portion of the fluid may continue in motion without tendency to lateral spreading, while all other parts remain at rest. It is shown,—1, that the motion in this filament of fluid may be propagated with a uniform velocity; 2, that in one straight line, which may be called its axis, the motion is entirely longitudinal; 3, that at all other points the motion is partly longitudinal and partly transversal; 4, that the motion is vibratory, the vibrations both longitudinal and transversal following the law of sines; 5, that the condensation (s) in any transverse plane, at a point whose co-ordinates in that plane reckoned from the axis are x and y, is given by the equation

$$\frac{d^2s}{dx^2}+\frac{d^2s}{dy^2}+gs=0,$$

g being a certain constant. It follows that the condensation in any transverse plane, being determined by a partial differential equation, is arbitrary, and by consequence that the transverse velocity varies at a given time from point to point of any transverse plane in an arbitrary manner. To obtain the foregoing equation, it is assumed that the condensation at any point of a transverse plane, has to the condensation at the intersection of the plane with the axis, a ratio not variable with the time.

Each fluid filament in vibration is supposed in this theory to correspond to a *ray* of light. The vibrations in different fluid filaments may co-exist, and consequently rays be propagated in the same direc-

tion independently of each other. A ray of common light has the condensation symmetrically arranged about the axis.

May 25, 1846.

A Theory of the Polarization of Light on the Hypothesis of Undulations. By the same Author.

This paper is a continuation of the foregoing. A ray in which the condensation is not arranged symmetrically about the axis is considered to be *polarized*. Polarization in this theory corresponds to difference of condensation in different directions transverse to the axis of the ray. The sensation of light is due to the *transverse* vibrations. By assuming that the bifurcation of a ray takes place so that the transverse velocity at each point is resolved into two velocities at right angles to each other, and that these are respectively the velocities at the corresponding points of the two parts into which the ray is divided, Professor Challis finds,—1, that if the original ray be one of common light, the two parts are symmetrical about planes at right angles to each other passing through the axis, and are each of half the intensity of the original ray; 2, that if the original ray has been once polarized, the ratio of the two parts is equal to the square of the tangent of the angle which the plane of the second polarization makes with that of the first; 3, that whether the original ray be one of common light or a polarized ray, the two parts, on pursuing the same path, form a compound ray the intensity of which is independent of the difference of phase. According to this theory, elliptically or circularly polarized light is produced whenever a ray of first polarization is divided into two parts which subsequently pursue the same path in different phases. If the parts be made to meet in the same phase, they constitute the original polarized ray. Hence is explained the necessity of the analysing plate for the production of colours by polarized rays transmitted through thin pieces of uniaxal or biaxal crystals. The compound rays, if received directly by the eye on leaving the crystal, would be of the same intensity whatever be the difference of phase. But when they fall on the plate, those incident in the same phase, being equivalent to rays of first polarization, are incapable of reflexion, while the remainder, which are incident in the form of elliptically or circularly polarized light, are reflected in different degrees of intensity according to the difference of phase. The author states that he has extended this theory to the phænomena of double refraction.

On a Change in the State of Vision of an Eye affected with a mal-formation. By G. B. Airy, Esq., Astronomer Royal.

Twenty years ago, the author communicated to the Society a statement of the effects of a mal-formation in his left eye. The rays of light coming from a luminous point, and falling on the whole surface

of the pupil, do not converge to a point at any position within the eye, but converge so as to pass through two lines at right angles to each other, and, in the ordinary position of the head, inclined to the vertical, as formerly described (Transactions of the Society, vol. ii.). As the luminous point is moved further from or nearer to the eye, the image of the point becomes a straight line in one or other of the positions above-mentioned. Since 1825 the inclinations of the two focal lines to the vertical, their length, and their sharpness do not appear to have undergone any sensible change, but the distances at which the luminous point must be placed to bring the focal lines respectively exactly upon the retina are increased, having been formerly 3·5 and 6 inches, and being now 4·7 and 8·9 inches. Thus while the shortsightedness of the eye is diminished the astigmatism remains the same.

On the Geometrical Representation of the Roots of Algebraic Equations. By the Rev. H. Goodwin, late Fellow of Caius College, and Fellow of the Cambridge Philosophical Society.

The changes of value of any function of x, $f(x)$, may be very clearly, and for some purposes very usefully represented, by tracing the curve defined by the equation $z=f(x)$; and the positive and negative roots of the equation $f(x)=0$ will be the distances from the origin at which the curve cuts the axis of x.

In this memoir a similar method is applied to the representation of the changes of value of a function of (x), corresponding not only to real values of x, but also to values of the form $x+y\sqrt{-1}$. If we make $z=f(x+y\sqrt{-1})$, and restrict ourselves to real values of z, the equation separates itself into two, which, it is shown, may be represented symbolically by

$$z=\cos\left(y\frac{d}{dx}\right)f(x)$$

$$\text{and } 0=\sin\left(y\frac{d}{dx}\right)f(x),$$

and these will correspond to a curve of double curvature, the intersections of which with the plane of xy will determine by the distance of those points from the origin the imaginary roots of the equation $f(x)=0$.

The properties of this curve are fully discussed for the case of $f(x)$ being equivalent to $x^n+p_1x_{n-1}+p_2x_{n-}+\ldots\ldots+p_n$, where $p_1\,p_2\ldots\ldots p_n$ are real; and the following results are obtained.

1. The ordinate of the curve admits of no maximum or minimum value.

2. The curve goes off into infinite branches, which lie in asymptotic planes equally inclined to each other, and which tend alternately to positive and negative infinity.

3. Any plane parallel to the plane of xy cuts the curve in n points and no more.

From this last result the existence of n roots and no more for an equation of n dimensions is the immediate result.

Several well-known theorems are deduced from this view of the subject, and are given as illustrations.

The actual curves are traced, corresponding to the various cases of the quadratic, the cubic, and the biquadratic equations, and to the equation $x^n - 1 = 0$.

In the conclusion of the memoir it is remarked that the results obtained are not exclusively applicable to the case of algebraic equations, and the methods are applied to the case of $f(x) = sin\ x$.

The author trusts that the contents of this memoir, though not adding to the number of known theorems, may yet be useful as putting the subject in a new light, and as furnishing a method of demonstrating the existence of the roots of algebraic equations more simple and direct than any other which he has seen.

Cases of Morbid Rhythmical Movements, with observations. By G. E. Paget, M.D., Fellow of Caius College and of the Royal College of Physicians, London.

Seven cases were related. The movements were vibratory, rotatory, bowing, &c. In some of the cases they were incessant; in others paroxysmal; and in others again they were of both kinds, the predominant movement being replaced at intervals by distinct paroxysms.

On a comparison of these cases with the few others on record, and with the experiments of Flourens and Majendie, it was inferred as probable, that one class of the movements, viz. the rotatory, depended on disorder in the cerebellum or its transverse commissure, the pons. With regard to the other movements, it appeared that there were no sufficient grounds for even a probable conjecture as to the particular part of the encephalon, the excitement or disorder of which might act as an immediate cause of the movements.

The *remote* causes were such as under other circumstances are known to excite the common convulsive diseases, such as chorea anp epilepsy. These remote causes were in most cases *eccentric*.

November 9, 1846.

On the Structure of the Syllogism, and on the application of the Theory of Probabilities to Questions of Argument and Authority. By Professor De Morgan.

The object of this paper is twofold: first, to establish two distinct theories of the syllogism, both differing materially from that of Aristotle, and each furnishing a general canon for the detection of all its legitimate forms of inference; secondly, to investigate the mode in which the distinctive character of the two great sources of conviction, *argument* and *authority*, affects the application of the notion of probability to questions not admitting of absolute demonstration.

The two theories of the syllogism arise out of simple notions connected with the *forms* of propositions and their *quantities*. The difference between a positive and negative assertion is not essential, but depends on the manner in which objects of thought are described by language. If Y and y be names so connected that each contains everything which is not in the other, and the two have nothing in common (a relation which is described by calling them *contrary* names), the propositions 'Every X is Y' and 'no X is y' are simply identical. In the same manner, the particular and universal proposition are only accidentally distinct. If in 'some Xs are Ys' the Xs there specified had had a name belonging to them only, say Z, then the preceding proposition would have been identical in meaning with 'every Z is Y.'

From the above it is made to follow, that every legitimate syllogism can be reduced to one of universal affirmative premises, either by introduction of contrary terms, or invention of subgeneric names.

In considering the nature of the simple proposition, Mr. De Morgan uses a notation proposed by himself. Thus—

Every X is Y is denoted by	X)Y	A
No X is Y	X.Y	E
Some Xs are Ys .. .	XY	I
Some Xs are not Ys ..	X:Y	O

and names which are contraries are denoted by large and small letters. Aristotle having excluded the contrary of a name from formal logic, and having thereby reduced the forms of proposition to four, these forms (*universal affirmative, universal negative, particular affirmative, particular negative*) the writers on logic in the middle ages represented by the letters A, E, I, O. Thus X)Y and Y)X are equally represented by A. When contraries are expressly introduced, all the forms of assertion or denial which can obtain between two terms and their contraries, are *eight* in number; and the most convenient mode of representing them is as follows :—Let the letters A, E, I, O have the above meaning, but only when the order of subject and predicate is XY. Then let a, e, i, o stand for the same propositions, after x and y, the contraries, are written for X and Y. The complete system then is—

A=X)Y	a=x)y=Y)X
O=X:Y	o=x:y=Y:X
E=X.Y	e=x.y
I=XY	i=xy

and every form in which subject and predicate are in any manner chosen out of the four X, Y, x, y, so that one shall be either X or x, and the other either Y or y, is reducible to one or other of the preceding.

The propositions e and i, which are thus newly introduced, are only expressible as follows, with reference to X and Y.

(*i.*) *There are things which are neither* X *nor* Y.

(*e.*) *There is nothing but is either* X *or* Y *or both.*

The connexion of these eight forms is fully considered, and the

various syllogisms to which they lead. Rejecting every form of syllogism in which as strong a conclusion can be deduced from a weaker premise; rejecting, for instance,

$$Y)X+Y)Z=XZ$$

because XZ equally follows from $Y)X+YZ$, in which YZ is *weaker* than Y)Z—all the forms of inference are reduced to three sets.

1. A set of two, called *single* because the interchange of the terms of the conclusion does not alter the syllogism. Neither of these forms are in the Aristotelian list. One of them is

$$X)Y+Z)Y=xz;$$

or *if every* X *be a* Y, *and also every* Z, *then there are things which are neither* X *nor* Z; *namely, all which are not* Ys.

2. A set of six, in which the interchange produces really different syllogisms of the same form, and in which both premises and conclusion can be expressed in terms of *three names*, without the contrary of either. This set includes the whole Aristotelian list, except those in which a weaker premise will give as strong a conclusion, or the one in which the same premises will give a stronger conclusion.

3. A set of six resembling the last in everything but this, that no one of them is expressible without the new forms *e* and *i*; that is, requiring three names and the contraries of one or more of them.

Those of the third set are not reducible to Aristotelian syllogisms, as long as the eight standard forms of assertion are adhered to.

The second theory of the syllogism has its principles laid down in the memoir before us; but those principles are only applied to the evolution of the cases which are not admitted into the Aristotelian system. The formal statement of the manner in which the ordinary cases of syllogism are connected with those peculiar to this second system is contained in an *Addition*.

In providing that premises shall certainly furnish a conclusion, the common system requires that one at least of the premises shall speak *universally* of the middle term; that is, shall make its assertion or denial of *every* object of thought which is named by the middle term. Mr. De Morgan points out that this is not necessary: m being the fraction of all the cases of the middle term mentioned in one premise, and n in the other, all that is necessary is that $m+n$ should be greater than unity. In such case, the real middle term, being the collection of all the cases by comparison of which with other things inference arises, is the fraction $m+n-1$ of all the possible cases of the middle term. Thus, from the premises 'most Ys are Xs' and 'most Ys are Zs,' it can be inferred that some Xs are Zs, since m and n are both greater than one-half. The assignment of definite quantity to the middle term in both premises, gives a canon of inference, of which the Aristotelian rule is only a particular case.

In the addition above alluded to, this same canon, namely 'that more Ys in number than there exist separate Ys shall be spoken of in both premises together,' is made to take the following form:—If in an affirmation or negation, in 'As are Bs' and 'As are not Bs,' definite numerical quantity be given to both subject and predicate, if

it oe stated how many As are spoken of and how many Bs—the number of *effective* cases of the middle term is seen to be the number of *subjects* in an affirmative proposition, whether the middle term be subject or predicate. Hence, defining the effective number of a premise to be the number of subjects if the proposition be affirmative, and the number of cases of the middle term if it be negative, all that is necessary for inference (over and above the usual condition that both premises must not be negative) is that the sum of the effective numbers of the two premises shall exceed the number of existing cases of the middle term; and the excess (being the fraction denoted by $m+n-1$ in the Memoir) gives the number of cases in which inference can be made.

To attempt to combine these two systems of *form* and of *quantity* is rendered useless by language not possessing the forms of mixed assertion and denial, which the syllogisms deduced from the combination would require. As far as the combination can, in Mr. De Morgan's opinion, be made, nothing is required but a distinct conception of, and nomenclature for, the usual modes of expressing a logical form, and implying one or the other of the alternations which the mere expression leaves unsettled. Mr. De Morgan proposes the following language.

Two names are *identical* when each contains all that the other contains: but when all the first (and more) is contained in the second, then the first is called a *subidentical* of the second, and the second a *superidentical* of the first. Two names are contrary when everything (or everything intended to be spoken of) is in one or the other and nothing in both. But when the two names have nothing in common, and do not between them contain everything, they are called *subcontraries* of one another. And again, if everything be in one or the other, and some things in both, they are called *supercontraries* of one another. Lastly, if the two names have each something in common and something not in common, and moreover do not between them contain everything, each is called a *complete particular* of the other. A table is then given, which contains every form of complex syllogism.

If X and Z be the terms of the conclusion, and both be described in terms of Y, the middle term: it can be seen from this table what can be affirmed and what denied, of X with respect to Z. For instance, if X be supercontrary of Y, and Z subcontrary, then X must be a superidentical of Z: but if X and Z be both subidenticals of Y, nothing can be affirmed; only it may be denied that X is either contrary or supercontrary of Z.

The remaining part of this paper relates to the application of the theory of probabilities above-mentioned. Mr. De Morgan asserts that no conclusion of a definite amount of probability can be formed from argument alone; but that all the results of argument must be modified by the testimony to the conclusion which exists in the mind, whether derived from the authority of others, or from the previous state of the mind itself. The foundation of this assertion is the circumstance that the insufficiency of the argument is no index of

the falsehood of the conclusion. Various cases are examined; but it must here be sufficient to cite one or two results.

If μ be the probability which the mind attaches to a certain conclusion, a the probability that a certain argument is valid, and b the probability that a certain argument for the contradiction is valid: then the probability of the truth of the conclusion is

$$\frac{(1-b)\mu}{(1-b)\mu+(1-a)(1-\mu)}.$$

If $b=0$, or if there be no argument against, and if the mind be unbiassed, or if $\mu=\frac{1}{2}$, this becomes

$$\frac{1}{2-a} \text{ or } a+\frac{(1-a)^2}{2-a}.$$

For this writers on logic generally substitute a, confounding the absolute truth of the conclusion with the validity of the argument, and neglecting the possible case of the argument being invalid, and yet the conclusion true.

November 23, 1846.

On a New Notation for expressing various Conditions and Equations in Geometry, Mechanics and Astronomy. By the Rev. M. O'Brien.

If A, P, P′ be any three points in space, whether in the same straight line or not, and if the lines AP and AP′ be represented in magnitude and direction by the symbols u and u', then, according to principles now well-known and universally admitted, the line PP′ is represented in magnitude and direction by the symbol $u'-u$. Now if AP and AP′ be equal in magnitude, and make an indefinitely small angle with each other, PP′ is an indefinitely small line at right angles to AP, and $u'-u$ becomes du. Hence it follows, that, if u be the symbol of a line of invariable magnitude, du is the symbol of an indefinitely small line at right angles to it; and therefore, if λ be any arbitrary coefficient, λdu is the general expression for a right line perpendicular to u.

The sign λd therefore indicates perpendicularity, when put before the symbol of a line of invariable length. The object of the author is to develope this idea, and to show that it not only leads to a simple method of expressing perpendicularity, but also furnishes a notation of considerable use in expressing various conditions and equations in geometry, mechanics, astronomy, and other sciences involving the consideration of *direction* and *magnitude*.

The author first reduces the sign λd to a more convenient form, which not only secures the condition that u is invariable in length, but also defines the magnitude and direction of the perpendicular which λdu denotes. This he does in the following manner. He assumes

$$u=x\alpha+y\beta+z\gamma,$$

(where $\alpha\ \beta\ \gamma$ represent three lines, each a unit in length, drawn at right angles to each other, and $x\ y\ z$ are any arbitrary numerical coefficients,) and supposes that the differentiation denoted by d affects $\alpha\ \beta\ \gamma$, but not $x\ y\ z$. This secures the condition that u is invariable in length, and leads to the following expression for λdu, viz.

$$\lambda du=(zy'-z'y)\alpha+(xz'-x'z)\beta+(yx'-y'x)\gamma,$$

$x'\ y'\ z'$ being arbitrary coefficients.

Assuming $u'=x'\alpha+y'\beta+z'\gamma$, it appears from this expression for λdu, that $du=0$ when $u=u'$, and therefore that d denotes a differential taken on the supposition that u' is constant.

On this account the author substitutes the symbol $D_{u'}$ in place of λd; he then shows that the operation $D_{u'}$ is *distributive* with respect to u' (*i. e.* that $D_{u'+u''}=D_{u'}+D_{u''}$), and to indicate this he elevates the subscript index u', and writes $Du'.u$ instead of $D_{u'}u$. Thus he obtains the expression

$$Du'.u=(zy'-z'y)\alpha+(xz'-x'z)\beta+(yx'-y'x)\gamma.$$

From this it follows that $Du'.u$ is a line perpendicular both to u' and u, and that the numerical magnitude of $Du'.u$ is $rr'\sin\theta$, where r and r' are the numerical magnitudes of u and u', and θ the angle made by u and u'.

Having investigated the principal properties of the operation $Du'.$, the author, by a similar method, obtains another notation, $\Delta u'.u$, which represents the expression $xx'+yy'+zz'$, or $rr'\cos\theta$. He then gives some instances of the application of these two notations to mechanics, which may be briefly stated as follows:—

1st. If U, U′, U″, &c. be the symbols* of any forces acting upon a rigid body, and u, u', u'', &c. the symbols† of their respective points of application, then the six equations of equilibrium are included in the two equations

$$\Sigma U=0 \text{ and } \Sigma Du.U=0.$$

2nd. That these two equations are the necessary and sufficient conditions of equilibrium, may be proved very simply from first principles by the use of the notation Du.

3rd. The theory of couples is included in the equation $\Sigma Du.U=0$. In fact the symbol $Du.U$ expresses, in magnitude and direction, the axis of the couple by which the force U is transferred from its point of application U to the origin.

4th. Supposing that the forces U, U′, U″, &c. do not balance each other, and putting $\Sigma U=V$, $\Sigma Du.U=W$, we may show immediately, by the use of the notation Δu, that the condition of there being a single resultant is

$$\Delta V.W=0;$$

and when there is not a single resultant, the axis of the couple of minimum moment is

* By the symbol of a force is meant the expression $X\alpha+Y\beta+Z\gamma$, where X Y Z are the three components of the force.

† By the symbol of a point is meant the expression $x\alpha+y\beta+z\gamma$, where $x\ y\ z$ are the coordinates of the point.

$$\frac{\Delta V.W}{\Delta V.V.}.V.$$

5th. The three equations of motion of a rigid body about its centre of gravity are included in the equation

$$\frac{d}{dt}\left(\Sigma Du.\frac{du}{dt}\delta m\right)=\Sigma Du.U\delta m\,; \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

u being the symbol of the position of any particle δm of the body, and U the symbol of the accelerating force acting on δm.

6th. If ω be assumed to represent the expression $\omega_1\alpha+\omega_2\beta+\omega_3\gamma$, where ω_1, ω_2, ω_3 are the angular velocities of the planes of yz, zx, xy about the axes of x, y, z respectively, then the symbol of the velocity of δm is $D\omega.u$; from which follow immediately the three well-known equations,

$$\frac{dx}{dt}=\omega_2 z-\omega_3 y,\quad \frac{dy}{dt}=\omega_3 x-\omega_1 z,\quad \frac{dz}{dt}=\omega_1 y-\omega_2 x.$$

The symbol ω represents in direction the axis of instantaneous rotation, and in magnitude the angular velocity about that axis.

7th. The equation (1.) may be reduced to the form

$$\frac{d.}{dt}\{A\omega_1\alpha+B\omega_2\beta+C\omega_3\gamma\}=\Sigma Du.U\delta m,$$

which includes Euler's three equations of motion about a fixed point.

8th. If the forces U, U′, U″, &c. arise from the attraction of a distant body, the symbol of whose position is u', this equation may be further reduced to the form

$$\frac{d}{dt}\left(A\omega_1\alpha+B\omega_2\beta+C\omega_3\gamma\right)=\frac{3m'}{r'^5}\,Du'.(Ax'\alpha+By'\beta+Cz'\gamma).$$

9th. In the case of the earth attracted by the sun or moon, this equation becomes

$$\frac{d\omega}{dt}=\frac{3m'}{r'^5}\,\lambda(\Delta u'.\gamma)(Du'.\gamma)\,;$$

γ being the polar axis, and $\lambda=\dfrac{C-A}{A}$.

10th. The mean daily motion of γ is given by the equation

$$\frac{d\gamma}{dt}=\frac{3m'}{nr'^5}\,\lambda(\Delta u'.\gamma)(Du'.\gamma)\,;$$

which equation gives immediately all the well-known expressions for solar and lunar precession and nutation, for $\dfrac{d\gamma}{dt}$ is the symbol of the velocity of the north pole, representing that velocity both in magnitude and direction. END

Supplement to a Memoir on some cases of Fluid Motion. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

In a former paper the author had given the mathematical calcula-

tion of an instance of fluid motion, which seemed to offer an accurate means of comparing theory and observation in a class of motions, in which, so far as the author is aware, they had not been hitherto compared. The instance referred to is that in which a vessel or box of the form of a rectangular parallelepiped is filled with fluid, closed, and made to perform small oscillations. It appears from theory that the effect of the inertia of the fluid is the same as that of a solid having the same mass, centre of gravity and principal axes, as the solidified fluid, but different principal moments of inertia. In this supplement the author gave a series for the calculation of the principal moments, which is more rapidly convergent than one which he had previously given. It is remarkable that these series, though numerically equal, appear under very different forms, the nth term of the latter containing exponentials of the forms $\varepsilon^{n\pi x}$ and $\varepsilon^{\frac{n\pi}{x}}$, while the nth term of the former contains exponentials of the second form only. In conclusion, the author referred to some experiments which he had performed with a box, such as that described, filled with water, employing the method of bifilar oscillations. The moment of inertia of the fluid about an axis passing through its centre of gravity (*i. e.* the moment of inertia of the imaginary solid which may be substituted for the fluid), was a little greater as determined by experiment than as determined by theory, as might have been expected, since the friction of the fluid was not considered in the calculation. The difference between theory and experiment varied in different cases from the $\frac{1}{13}$th to the $\frac{1}{21}$st part of the whole quantity.

December 7, 1846.

On the Principle of Continuity in reference to certain results of Analysis. By Professor Young of Belfast College.

The object of this paper is to inquire into the influence of the law of continuity, as it affects the extreme or ultimate values of variable functions, more especially those involving infinite series and definite integrals.

The author considers that this influence has hitherto been improperly overlooked; and that to this circumstance is to be attributed the errors and perplexities with which the different theories of those functions are found to be embarrassed. He shows that every particular case of a general analytical form—even the ultimate or limiting case—must come under the control of the law implied in that form; this law being equally efficient throughout the entire range of individual values. Except in the limiting cases, the law in question is palpably impressed on the several particular forms; but at the limits it has been suffered to escape recognition, because indications of its presence have not been actually preserved in the notation.

It is in this way that the series $1-1+1-1+$ &c. has been confounded with the limits of the series $1-x+x^2-x^3+$ &c.; these

limits being arrived at by the continuous variation of x from some inferior value up to $x=1$, and from some superior value down to $x=1$. It is shown however that the series $1-1+$ &c. has no equivalent among the individual cases of $1-x+x^2-$ &c., with which latter, indeed, it has no connexion whatever.

By properly distinguishing between the real limits, and what is generally confounded with them, the author arrives at several conclusions respecting the limiting values of infinite series directly opposed to those of Cauchy, Poisson, and others. And to prevent a recurrence of errors arising from a neglect of the distinction here noticed, he proposes to call such an isolated series as $1-1+1-$&c. *independent* or *neutral*; and the extreme cases of $1-x+x^2-$&c., *dependent* series: the difference between a dependent and a neutral series becomes sufficiently marked, as respects notation, by introducing into the former what the author calls the *symbol of continuity*, which indeed is no other than the factor, whose ascending powers Poisson introduces—and, as here shown, unwarrantably—into the successive terms of strictly *neutral* series; thus bringing such series under the control of a law to which in reality they owe no obedience.

An error somewhat analogous to this is shown to be committed in the treatment of certain definite integrals, which are here submitted to examination and correction, and some disputed and hitherto unsettled points in their theory fully considered. The author is thus led to what he considers an interesting fact in analysis; viz. that the *differentials* of certain forms require *indeterminate corrections*, in a manner similar to that by which *determinate* corrections are introduced into *integrals*; and he attributes to the neglect of these the many erroneous summations assigned to certain trigonometrical series. This is illustrated by a reference to the processes of Poisson.

The paper concludes with some observations on what has been called *discontinuity*; a term which the author thinks is sometimes injudiciously employed in analysis, and prefers to treat discontinuous functions as implying distinct continuities; and by considering these in accordance with the principles established in the former part of the paper, he arrives at results for definite integrals of the form $\int_{-m}^{+n} x^{-p}\,dx$ totally different from those obtained by Poisson. Two notes are appended to the paper; one explaining what the author denominates *insensible convergency* and *insensible divergency*, and the other discussing some conclusions of Abel in reference to certain trigonometrical developments.

March 1, 1847.

On the Theory of Oscillatory Waves. By G. G. Stokes, M.A., Fellow of Pembroke College.

The waves which form the subject of this paper are characterized

by the property of being propagated with a constant velocity, and without degradation, or change of form of any kind. The principal object of the paper is to investigate the form of these waves, and their velocity of propagation, to a second approximation; the height of the waves being supposed small, but finite. It is shown that the elevated and depressed portions of the fluid are not similar, as is the case to a first approximation; but the hollows are broad and shallow, the elevations comparatively narrow and high. The velocity of propagation is the same as to a first approximation, and is therefore independent of the height of the waves. It is remarkable that the forward motion of the particles near the surface is not exactly compensated by their backward motion, as is the case to a first approximation; so that the fluid near the surface, in addition to its motion of oscillation, is flowing with a small velocity in the direction in which the waves are propagated; and this velocity admits of expression in terms of the length and height of the waves. The knowledge of this circumstance may be of some use in leading to a more correct estimate of the allowance to be made for leeway in the case of a ship at sea. The author has proceeded to a third approximation in the case in which the depth of the fluid is very great, and finds that the velocity of propagation is increased by a small quantity, which bears to the whole a ratio depending on the square of the ratio of the height of the waves to their length.

In the concluding part of the paper is given the velocity of propagation of a series of waves propagated along the common surface of two fluids, of which the upper is bounded by a horizontal rigid plane. There is also given the velocity of propagation of the above series, as well as that of the series propagated along the upper surface of the upper fluid, in the case in which the upper surface is free. In these investigations the squares of small quantities are omitted.

March 15, 1847.

Contributions towards a System of Symbolical Geometry and Mechanics. By the Rev. M. O'Brien.

The distinction which has been made by an eminent authority in mathematics between *arithmetical* and *symbolical* algebra, may be extended to most of the sciences which call in the aid of algebra. Thus we may distinguish between *symbolical geometry* and *arithmetical geometry*, *symbolical mechanics* and *arithmetical mechanics*. This distinction does not imply that in one division numbers only are used, and in the other symbols, for symbols are equally used in both; but it relates to the degree of generality of the symbolization. In the arithmetical science, the symbols have a purely numerical signification; but in the symbolical they represent, not only abstract quantity, but also all the circumstances which, as it is expressed, *affect* quantity. The arithmetical science is in fact the first step of generalization, the symbolical is the complete generalization.

In this view of the case, the author has entitled his paper Contributions towards a System of Symbolical Geometry and Mechanics. The proposed geometrical system consists, first, in representing curves and surfaces, not by equations, as in the Cartesian method, but by *single symbols*; and secondly, in using the *differential notation* proposed in a former paper* to denote *perpendicularity*, and to express various equations and conditions. The proposed mechanical system is analogous in many respects. Examples of it have already been given in the paper just quoted.

The author uses the term *direction unit* to denote a line of a unity of length drawn in any particular direction; and he employs the symbols α β γ to denote any three direction units at right angles to each other.

He defines the position of any point P in space by the symbol representing the line OP (O being the origin) in magnitude and direction. If x y z be the numerical values of the coordinates of P, and α β γ the direction units of the coordinate axes, the expression

$$x\alpha + y\beta + z\gamma$$

represents the line OP in magnitude and direction, and therefore defines the position of P. This expression he calls the *symbol* of the point P.

If r be the numerical magnitude, and ε the direction unit of OP, we have

$$r\varepsilon = x\alpha + y\beta + z\gamma:$$

$r\varepsilon$ is therefore another form for the symbol of the point P.

The following is the method by which the author represents curves and surfaces.

If the symbol of a point involves an arbitrary quantity, or, as it is called, a variable parameter, the position of the point becomes indeterminate, but so far restricted that it will be always found on some line or curve. Hence the symbol of a point becomes the symbol of a line or curve when it involves a variable parameter.

In like manner, when the symbol of a point involves *two* variable parameters, it becomes the symbol of a surface.

The parameters here spoken of are supposed to be numerical quantities. An arbitrary direction unit is clearly equivalent to two such parameters; and therefore, when the symbol of a point involves an arbitrary direction unit, it becomes the symbol of a surface.

The following are examples of this method :—

1. If u be the symbol of any particular point of a right line whose direction unit is ε, then the symbol of that right line is

$$u + r\varepsilon,$$

r being arbitrary.

2. If u be the symbol of the centre of a sphere, and r its radius, the symbol of the surface of a sphere is

$$u + r\varepsilon,$$

ε being an arbitrary direction unit.

* Read Nov. 23, 1846. P 33

3. If u be the symbol of any particular point of a plane, ε and ε' the direction units of any two lines in the plane, the symbol of the plane is

$$u+r\varepsilon+r'\varepsilon',$$

r and r' being arbitrary.

4. If ε be the direction unit and r the numerical magnitude of the perpendicular from the origin on a plane, the symbol of the plane is

$$r\varepsilon+\mathrm{D}v.\varepsilon,$$

v being an arbitrary line symbol, *i. e.* denoting in magnitude and direction any arbitrary line.

5. If u and u' be the symbols of two points, the symbol of the right line drawn through them is

$$u+m(u'-u),$$

m being arbitrary.

6. If u be the symbol of any curve in space, the symbol of the tangent at the point u is

$$u+mdu,$$

m being arbitrary.

7. The symbol of the osculating plane at the point u is

$$u+mdu+m'd^2u,$$

m and m' being arbitrary.

8. If s denotes the length of the arc of the curve, and ε the direction unit of the tangent, then

$$\varepsilon=\frac{du}{ds}.$$

9. $\frac{d\varepsilon}{ds}$ or $\frac{1}{ds}\,d\left(\frac{du}{ds}\right)$ represents a line equal to the reciprocal of the radius of curvature drawn from the point u towards the centre of curvature, *i. e.* it represents what may be called the *index of curvature* in magnitude and direction.

Hence, since $u=x\alpha+y\beta+z\gamma$, the numerical magnitude of $\frac{1}{ds}\,d\left(\frac{du}{ds}\right)$ is

$$\frac{1}{ds}\sqrt{\left\{\left(d\frac{dx}{ds}\right)^2+\left(d\frac{dy}{ds}\right)^2+\left(d\frac{dz}{ds}\right)^2\right\}},$$

which is the general expression for the reciprocal of the radius of curvature.

10. The symbol of the normal which lies in the osculating plane is

$$u+md\left(\frac{du}{ds}\right),$$

m being arbitrary.

11. The symbol of *any* normal at the point u, *i. e.* the symbol of the normal plane, is

$$u+\mathrm{D}v.du,$$

v being an arbitrary line symbol.

12. The symbol of the normal perpendicular to the osculating plane is

$$u+m\mathrm{D}d^2u.du,$$

m being arbitrary.

13. If u be the symbol of a surface, involving therefore two variable parameters, λ and μ suppose, then the symbol of the normal at the point u is

$$u+m\mathrm{D}\frac{du}{d\lambda}\cdot\frac{du}{d\mu},$$

m being arbitrary.

14. The symbol of the tangent plane at the point u is

$$u+mdu, \text{ or } u+m\frac{du}{d\lambda}+n\frac{du}{d\mu},$$

m and n being arbitrary.

15. The symbol of the plane which contains the three points $u\ u'\ u''$ is

$$u+m(u'-u)+n(u''-u).$$

16. If u be the symbol of a right line, the symbol of the plane containing it and the point u' is

$$u+m(u'-u).$$

The following are examples of the proposed mechanical system in addition to those given in the paper already quoted.

1. If r be the radius vector of a planet, and $\alpha\ \beta\ \gamma$ be chosen so that α is the direction unit of the radius vector, and γ perpendicular to the plane of the orbit, it may be shown immediately by the symbolical method, that the symbol of the force acting on the planet is

$$\left(\frac{d^2r}{dt^2}-r\omega^2\right)\alpha+\frac{1}{r}\frac{d(r^2\omega)}{dt}\beta+r\omega\omega'\gamma,$$

where ω is the angular velocity of the planet, and ω' that of the plane of the orbit about the radius vector. The expressions for the three component forces along r, perpendicular to r, and perpendicular to the plane of the orbit, are the coefficients of $\alpha\ \beta\ \gamma$ in this expression.

2. The equation of motion of the planet, when the force is the attraction of a fixed centre varying as the inverse square of the distance, is

$$\frac{d^2u}{dt^2}=-\frac{\mu\alpha}{r^2}.$$

It is curious that this equation is *immediately* integrable, the integral being the two equations

$$r^2\omega=h;$$

$$\frac{h}{r}=\frac{\mu}{h}+e\Delta\beta.\varepsilon.$$

The latter equation is the symbolical equation of a conic section,

the origin being focus, h c and ε being the arbitrary constants introduced by integration.

3. The application of this method to the case of a planet acted on by a disturbing force is worthy of particular notice, as it expresses the variations of the elements of the orbit with great facility, in the following manner :—

If U be the symbol of the disturbing force, we have

$$\frac{d(h\gamma)}{dt} = \mathrm{D}u.\mathrm{U} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$\frac{d(e\varepsilon)}{dt} = \frac{\mu r}{h^2}\beta\Delta\beta.\mathrm{U} + \mathrm{U}. \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

These two equations determine with great facility all the elements of the orbit. For γ is a direction unit perpendicular to the plane of the orbit (*i. e.* it is the symbol of the pole of the orbit), and therefore it defines completely the position of the plane of the orbit. Also ε is a direction unit in the plane of the orbit at right angles to the axis major, and therefore it determines the position of the axis major; in fact the direction unit of the axis major is $\mathrm{D}\gamma.\varepsilon$. The letters h and e have their usual signification.

To find h and γ separately from (1.), suppose that we obtain by integration of (1.)

$$h\gamma = \mathrm{W};$$

then $h^2 = \Delta\mathrm{W}.\mathrm{W}$; and h being thus found, we have $\gamma = \frac{\mathrm{W}}{h}$. The same observation applies to (2.).

4. The expression for the parallax of the planet is

$$\frac{\mu}{h^2} + \Delta\beta.\int dt\left(\frac{\mu r}{h^2}\beta\Delta\beta.\mathrm{U} + \mathrm{U}\right).$$

These instances suffice to show the nature of the proposed symbolical method.

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On the Partitions of Numbers, on Combinations, and on Permutations. By Henry Warburton, M.P., F.R.S., F.G.S., Member of the Senate of the University of London; formerly of Trinity College, A.M.

The use made by Waring of the Partitions of numbers in developing the power of a polynome, induced the author to seek for some general and ready method of determining in how many different ways a given number can be resolved into a given number of parts. On his communicating the method described in article 5 of Section I. of this abstract, to Professor De Morgan, in the autumn of 1846, that gentleman intimated a wish that the author would turn his attention also to Combinations; and such was the origin of the researches which form the subject of the 2nd and 3rd sections.

I. *On the Partitions of Numbers.*

1. Let $[N, p_\eta]$ denote how many different ways there are of resolving the integer N into p integral parts, none less than η. Then

$$[N, p_\eta] = [N \pm p\theta, p_{\eta \pm \theta}]. \quad . \quad . \quad . \quad . \quad . \quad . \quad (I.)$$

2. Such of the p-partitions of N as contain η as a part, and no part less than η, are obtained by resolving $N-\eta$ into $p-1$ parts not less than η, and by adding η, as a pth part, to every such $(p-1)$-partition. That is,

$$[N, p_\eta] - [N, \underset{\eta+1}{p}] = [N-\eta, \underset{\eta}{p-1}]. \quad . \quad . \quad . \quad (II.)$$

3. In (II.), substitute $\eta+1$, $\eta+2$, &c. successively for η. The sum of the results is

$$[N, p_\eta] - [N, p_{\eta+\theta+1}] = \underset{z}{S}_0^\theta [N-\eta-sp, \underset{\eta}{p-1}]. \quad . \quad (III.)$$

In this expression, when $\theta = I^*\left(\frac{N}{p}\right) - \eta$, the term $[N, p_{\eta+\theta+1}]$ vanishes, and the formula then becomes analogous to one published

* $I\left(\frac{N}{p}\right)$ is employed to avoid the long phrase, "the integer nearest to and not exceeding $\frac{N}{p}$."

anonymously by Professor De Morgan in a paper printed in the fourth volume, p. 87, of the Cambridge Mathematical Journal.

4. In (II.), for $[N, p_{\eta+1}]$ substitute $[N-p\eta, p_1]$, and transpose the terms. Then

$$[N, p_\eta]-[N-\eta, p_{\underset{\eta}{-}}1]=[N-p\eta, p_1]; \quad . \quad . \quad (IV.)$$

and this leads to

$$[N-\eta, p_{\underset{\eta}{-}}1]-[N-2\eta, p_{\underset{\eta}{-}}2]=[N-p\eta, p_{\underset{\eta}{-}}1];$$

and that leads to the summation

$$[N, p_\eta]=\underset{z}{S}_0^p[N-p\eta, z_1]. \quad . \quad . \quad . \quad . \quad (V.)$$

The lower limit of z in (V.) is made 0, in order that the formula may comprehend the extreme case $[0, 0_1]=1$, analogous to the extreme case in Combinations.

5. After substituting 1 for η, the author applies formula (IV.) to determining in how many different ways N can be resolved into p parts not less than 1. Let $[N, p_1]$ be the term in a table of double entry corresponding to column N, line p, in the table. From the head, in line 0, of each of the columns 0, 1, 2, 3, &c., draw a diagonal, advancing one column and one line at a time. Take these diagonals one after another, and in each of them compute by formula (IV.) the terms situate on lines 0, 1, 2, 3, &c., one by one in succession. If N be the number at the head of the column from which any diagonal takes its departure, there will be only N terms to compute on that diagonal, the further terms being only repetitions of the term on the line N. For the diagonal in question intersects line N in column 2N; and, by formula V,

$$[2N, N_1]=\underset{z}{S}_0^N[N, z_1]$$

= the sum of all the terms in column N. But, moreover,

$$[2N+y, N_1+y]=\underset{z}{S}_0^N[N, z_1]$$

= the same constant. The leading property of the table, indicated by the formula

$$[N, p_1]=\underset{z}{S}_0^p[N-p, z_1],$$

is, that the term $[N, p_1]$ = the sum of all the terms in column $N-p$, from line 0 to line p inclusive. After the publication of the anonymous paper before referred to, Professor De Morgan discovered this theorem also, but he did not announce it*.

II. *On Combinations.*

1. In ordinary Combinations, the combining elements are of different kinds, and there is but one element of a kind: in the case here considered, there are different kinds of elements, and there may be many elements of a kind; and more than one element of a kind may enter into the same combination.

* The author has recently discovered an equivalent formula in p. 264 of Euler's Int. in An. Infinitorum; but investigated by a totally different method, and not applied as the author has applied it.

2. If u elements enter at a time into each combination, and the *kinds* are determinate in number, and their number is s, let $\left\{ \begin{matrix} u \\ s \end{matrix} \right\}$ denote how many different combinations can then be formed: if the *elements* are determinate in number, and their number is σ, let the number of the combinations which can then be constructed, be denoted by $\{u, \sigma\}$. If $\varphi(x)$ be any function of x, let $D^u \varphi(x)$ denote the coefficient of x^u in that function developed according to the powers of x.

3. The same things as before being assumed, let a given set of elements consist of α elements of the kind A, $+\beta$ elements of the kind B, + &c. Take the product, K, of the s geometrical progressions,

$$[1+Ax+A^2x^2+\ldots\ldots+A^\alpha x^\alpha],\ [1+Bx+B^2x^2+\ldots\ldots+B^\beta x^\beta], \&c.$$

Then K will be of the form,

$$1+S[A]x+S[A^2+AB]x^2+S[A^3+A\cdot B+ABC]x^3+\&c.,$$

and $D^u[K]$ will be of the form

$$S[A^pB^qC^r\ \&c.],$$

the last expression being an aggregate of terms of the form $A^pB^qC^r\ldots$, each containing a different combination of u of the given elements, and their sum comprehending all the possible combinations of those elements taken u at a time. Now, if A, B, C, &c. be each made equal to 1, K will become

$$k=[1+x+x^2+\ldots+x^\alpha][1+x+x^2+\ldots+x^\beta].\ \&c.;$$

each of the terms $A^p.B^q.C^r.$ &c. will become 1, and the *number* of all the terms of the form $A^pB^qC^r\ldots$ which $D^u[K]$ or $S[A^pB^qC^r\ldots]$ contains, that is to say, $\{u, \sigma\}$ will be represented by $D^u[k]$; which latter coefficient the author next proceeds to determine.

Now

$$\left.\begin{aligned} k=\frac{1-x^{\alpha+1}}{1-x}\cdot\frac{1-x^{\beta+1}}{1-x}.\ \&c.&=[1-x^{\alpha+1}][1-x^{\beta+1}]\ldots[1-x]^{-s} \\ &=[1-x^{\alpha+1}][1-x^{\beta+1}]\ldots S_u^{\infty}{}_0\left[\frac{s^{u|1}}{1^{u|1}}{}^{*}x^u\right] \end{aligned}\right\}\text{(VI.}$$

$$=[1-x^{\alpha+1}][1-x^{\beta+1}]\ldots\frac{1}{1^{s-1|1}}\,S_u^{\infty}{}_0\left[[u+1]^{s-1|1}x^u\right]\text{(VII.)}$$

For brevity, write u_1, α_1, β_1, &c. respectively, for $u+1$, $\alpha+1$, $\beta+1$, &c.; and also write $[1]$ for $[1-x]^{-s}$; $[2]$ for $[1-x^{\alpha_1}][1-x]^{-s}$; that is, for $[1-x^{\alpha_1}].[1]$; $[3]$ for $[1-x^{\alpha_1}][1-x^{\beta_1}][1-x]^{-s}$; that is, for $[1-x^{\beta_1}].[2]$, and so on. Then

$$D^u[2]=D^u[1]-D^{u-\alpha_1}[1];$$

and

$$D^u[3]=D^u[2]-D^{u-\beta_1}[2];$$

* According to the factorial notation, here used by the author, $s^{u'\pm 1}$ represents $s\,[s\pm 1][s\pm 2]\ldots[s\pm(u-1)]$.

and

$$D^u[4]=D^u[3]-D^{u-\gamma_1}[3]\text{; and so on; (VIII.)}$$

and the developed product of the binomes,

$$[1-x^{\alpha_1}],[1-x^{\beta_1}],[1-x^{\gamma_1}],\ \&c.;$$

that is to say,

$$\begin{aligned}&1-x^{\alpha_1}+x^{\alpha_1+\beta_1}-x^{\alpha_1+\beta_1+\gamma_1}+\ \&c.\\&-x^{\beta_1}+x^{\alpha_1+\gamma_1}-\&c.\\&-x^{\gamma_1}+x^{\beta_1+\gamma_1}\\&-\&c.\ +\&c.\end{aligned}$$

when multiplied into the development of $[1-x]^{-s}$, manifestly leads to the following formula:

$$\left.\begin{aligned}D^u[m]=D^u[1]-S\left[D^{u-\alpha_1}[1]\right]+S\left[D^{u-\alpha_1-\beta_1}[1]\right]\\-S\left[D^{u-\alpha_1-\beta_1-\gamma_1}[1]\right]+\&c.\end{aligned}\right\}\text{(VIII*.)}$$

where, since the powers of x, in (VI.) or (VII.) developed, are to be all positive, no expression of the form

$$(u-\alpha_1),\ (u-\alpha_1-\beta_1),\ (u-\alpha_1-\beta_1-\gamma_1),\ \&c.$$

is to be negative. Then by giving to

$$D^u[1],\ D^{u-\alpha_1}[1],\ D^{u-\alpha_1-\beta_1}[1],\ \&c.\ .\ .\ \text{(IX.)}$$

their respective values, we obtain the series of expressions:

$$D^u[1]=\frac{1}{1^{s-1|1}}u_1{}^{s-1|1}=\frac{s^{u|1}}{1^{u|1}}=\begin{bmatrix}u\\s\end{bmatrix}$$

where in all the kinds the elements are plural without limit; a formula given by Hirsch:

$$D^u[2]=\frac{1}{1^{s-1|1}}\left[u_1{}^{s-1|1}-[u_1-\alpha_1]^{s-1|1}\right]=\begin{bmatrix}u\\s\end{bmatrix}$$

where the elements A are limited in number to α, but those of the other $(s-1)$ kinds are plural without limit:

$$D^u[3]=\frac{1}{1^{s-1|1}}\begin{bmatrix}u_1{}^{s-1|1}-[u_1-\alpha_1]^{s-1|1}+[u_1-\alpha_1-\beta_1]^{s-1|1}\\-[u_1-\beta_1]^{s-1,1}\end{bmatrix}=\begin{bmatrix}u\\s\end{bmatrix}$$

where, moreover, the elements B are limited in number to β, but those of the other $(s-2)$ kinds are plural without limit: and so for the rest. The law of the terms being evident, they need not be continued further.

Example of (IX.). Given one element of 1 kind, two elements of a 2nd kind, three of a 3rd, and four of a 4th; and let $u=5$. Then

$$\{u,\sigma\}=\frac{1}{1.2.3}\left\{6.7.8\begin{matrix}-4.5.6\\-3.4.5+1.2.3\\-2.3.4\\-1.2.3\end{matrix}\right\}=22.$$

4. If $\alpha=\beta=\gamma=$&c., formula (IX.) becomes

$$\{u,\sigma\}=\frac{1}{1^{s-1|1}}\,\mathrm{S}^{1}_{\theta\;0}\binom{u_1}{\alpha_1}\left[(-1)^{\theta}\frac{s^{\theta|-1}}{1^{\theta|1}}\,[u_1-\theta\alpha_1]^{s-1|1}\right]. \quad \text{(X.)}$$

Example of formula (X.) Given seven kinds of elements, and three of each kind; and let $u=4$. Then

$$\{u,\sigma\}=\frac{1}{1.2.3.4.5.6}\Big[5.6.7.8.9.10-7.1.2.3.4.5.6\Big]=203.$$

5. If it is required to determine many, or all, of the terms of the series $\{0,\sigma\}$, $\{1,\sigma\}$, $\{2,\sigma\}$, ..., $\{\sigma,\sigma\}$, formulas (VIII.) suggest the following process for the determination of those terms. An example will best explain the process.

Given 1 element of one kind, 2 elements of a second kind, and 3 of a third kind. How many combinations can be formed from these elements, when taken 0, 1, 2, 3, 4, 5, 6 at a time, respectively?

Values of u	0	1	2	3	4	5	6
Coefficients of x^u in $[1-x]^{-3}$	1	3	6	10	15	21	28
Multiply by $[1-x^2]$; that is, subtract	...	...	1	3	6	10	15
Coefficients of x^u in $[1-x^2][1-x]^{-3}$	1	3	5	7	9	11	13
Multiply by $[1-x^3]$; that is, subtract	...	...	...	1	3	5	7
Coefficients of x^u in $[1-x^2][1-x^3][1-x]^{-3}$	1	3	5	6	6	6	6
Multiply by $[1-x^4]$; that is, subtract	...	...	...	...	1	3	5
Coefficients of x^u in $[1-x^2][1-x^3][1-x^4]$ $[1-x]^{-3}$	1	3	5	6	5	3	1
That is	$\{0,\sigma\}$	$\{1,\sigma\}$	$\{2,\sigma\}$	$\{3,\sigma\}$	$\{4,\sigma\}$	$\{5,\sigma\}$	$\{6,\sigma\}$

6. Let a set of elements, S, such as we have been previously considering, consist of two similar sets, T and T′, which do not contain in common any elements of the same kind. If S consists of σ elements combining u at a time, and T consists of τ elements combining v at a time, T′ will consist of $(\sigma-\tau)$ elements combining $(u-v)$ at a time. Consider u as constant, for the moment, and v as variable. The author then shows that if by the process described in art. 5, the whole series of terms $\{v,\tau\}$ and the whole series of terms $\{u-v, \sigma-\tau\}$, have been determined, we can thence determine the whole series of terms $\{u,\sigma\}$ by means of the formula

$$\{u,\sigma\}=\mathrm{S}^{\tau}_{v\;0}\Big[\{v,\tau\}.\{u-v,\sigma-\tau\}\Big]; \quad . \quad . \quad \text{(XI.)}$$

and of this he gives examples.

7. In formula (XI.) substitute $(\sigma-u)$ for u; and develope $\{u,\sigma\}$ and $\{\sigma-u,\sigma\}$ in the manner indicated by that formula. By comparing the 1st, 2nd, 3rd, &c. terms respectively of $\{u,\sigma\}$ with the last, last but one, last but two, &c. terms of $\{\sigma-u,\sigma\}$, and *vice*

versâ, the author shows that $\{u, \sigma\}$ will be identical with $\{\sigma-u, \sigma\}$, provided $\{v, \tau\}$ is identical with $\{\tau-v, \tau\}$, and provided also $\{u-v, \sigma-\tau\}$ is identical with $\{\sigma-\tau-(u-v), \sigma-\tau\}$. But this identity actually exists when T consists of elements of one kind only, and when T′ also consists of elements of one kind only. For, in that case, every term of the series $\{v, \tau\}$ and every term of the series $\{u-v, \sigma-\tau\}$ is equal to 1. Let the elements of the single kind which T contains, be different from those of the single kind which T′ contains. Then the identity in question will exist, when S consists of elements, finite in number, of two different kinds: consequently, it exists also when T consists of elements, finite in number, of two different kinds, and T′ consists of elements, finite in number, of one or two other kind or kinds; that is, when S consists of elements, finite in number, of three or four different kinds. And therefore universally, in the case as well of finitely plural, as of singular elements, the following law obtains:

$$\{u, \sigma\} = \{\sigma-u, \sigma\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(XII.)}$$

Hence it follows that in applying formulas (IX.) and (X.) to particular cases, the labour of computation will be shortened by substituting for the variable the lesser of the two numbers u and $\sigma-u$.

8. The author next considers how many different combinations can be formed from a given set of elements, when every combination is to be constructed in conformity with a given type; in which type there are m different kinds containing v elements each, m' other different kinds containing v' elements each, m'' other different kinds containing v'' elements each, and so on; and where, consequently, in each combination, z, the number of kinds, is $m+m'+m''+$ &c.; and u, the number of elements, is $mv+m'v'+m''v''+$ &c. The type remaining constant, any combination conformable thereto may be altered, either by changing the particular z kinds which are selected out of the s given kinds; or, the kinds remaining the same, by altering the distribution of the parts $v, v, v, \ldots (m)v', v', v', \ldots (m')v'', v'', v'', \ldots (m'')$ &c., among those kinds. When all the elements are plural without limit, the changes of the former description will be represented by

$$\frac{s^{z}-1}{1^{z|1}};$$

and those of the latter description by

$$\frac{1^{z|1}}{1^{m|1}.\,1^{m'|1}.\,1^{m''|1}\ldots},$$

and their joint effect by the product

$$\frac{s^{z|-1}}{1^{z|1}} \times \frac{1^{z|1}}{1^{m|1}.\,1^{m'|1}.\,1^{m''|1}\ldots}. \quad . \quad . \quad . \quad . \quad \text{(XIII.)}$$

But when the elements of all the given kinds are finite in number, class these kinds, so that each kind in class 1 contains not fewer

than v elements; each kind in class 2 contains fewer than v, but not fewer than v' elements; each kind in class 3 contains fewer than v', but not fewer than v'' elements; and so on; and so that the given kinds may in this way be reduced, say, to t kinds containing v elements each $+ \mathrm{T}'$ kinds containing v' elements each $+ \mathrm{T}''$ kinds containing v'' elements each, &c. Then let $t - m + \mathrm{T}' = t'$; $t' - m' + \mathrm{T}'' = t''$; and so on. The given kinds being thus ordered, since we are required to select, 1st, m out of t kinds; then, 2nd, m' out of t' kinds; then, 3rd, m'' out of t'' kinds; and so on; the number of the different combinations which can be constructed from those kinds in conformity with the type, will be

$$\frac{t^{\underline{m|1}}}{1^{\underline{m|1}}} \cdot \frac{{}^{\prime}t^{\underline{m'|1}}}{1^{\underline{m'|1}}} \cdot \frac{{}^{\prime\prime}t^{\underline{m''|1}}}{1^{\underline{m''|1}}}, \text{ \&c.} \quad . \quad . \quad . \quad . \quad . \quad \text{(XIV.)}$$

If $tv + \mathrm{T}'v' + \mathrm{T}''v''$.. &c. is reduced to a single term, $t.v$; then formula (XIV.) becomes

$$\frac{t^{\underline{z|1}}}{1^{\underline{m|1}}.\, 1^{\underline{m'|1}}.\, 1^{\underline{m''|1}}.} \text{ \&c.} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(XV.)}$$

Example of (XIV.). Given eight elements of 1 kind, seven of a 2nd kind, six of a 3rd, five of a 4th, four of a 5th, three of a 6th, two of a 7th, and one element of an 8th kind, out of which it is required to construct combinations, each consisting of three kinds with five elements each + two kinds with three elements each + one kind with two elements. Of such combinations there can be formed

$$\frac{4^{\underline{3|1}}}{1^{\underline{3|1}}} \cdot \frac{3^{\underline{2|1}}}{1^{\underline{2|1}}} \cdot \frac{2^{\underline{1|1}}}{1^{\underline{1|1}}} = 24.$$

9. If it be required to determine how many different combinations can be constructed, each containing u elements of z kinds, and the given elements are all finite in number; we must form all the different z-partitions of u; and each of these partitions being regarded as a type, we must determine, by formula (XIV.) or (XV.), how many combinations correspond to each of these types; and the total number required will be the sum of all these particular determinations. But if the given elements may all be repeated without limit, it follows from formula (XIII.), that the sum of all the particular determinations may be represented by

$$\frac{s^{\underline{z|1}}}{1^{\underline{z|1}}} \times \mathrm{S}\left(\frac{1^{\underline{z|1}}}{1^{\underline{m|1}}.\, 1^{\underline{m'|1}}.\, 1^{\underline{m''|1}}.\text{ \&c.}}\right).$$

Now

$$\mathrm{S}\left(\frac{1^{\underline{z|1}}}{1^{\underline{m|1}}.\, 1^{\underline{m'|1}}.\, 1^{\underline{m''|1}}}\right)$$

denotes how many different permutations can be formed, when, in each different z-partition of u, the parts are permuted z together at a time; and the number of such permutations is

$$\Sigma^{z-1}[1] = \Sigma^{z-2}[u-1] = \frac{[u-1]^{\underline{z-1|1}}}{1^{\underline{z-1|1}}}.$$

Consequently the required sum is

$$\frac{s^{z|-1}}{1^{z|1}} \times \frac{[u-1]^{z-1|-1}}{1^{z-1|1}} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(XVI.)}$$

If in (XVI.) z varies from 0 to $u-1$,

$$\underset{z}{\mathrm{S}}{}_{0}^{u-1}\left[\frac{s^{z|-1}}{1^{z|1}} \times \frac{[u-1]^{z-1|1}}{1^{z-1|1}}\right] = \frac{s^{u|1}}{1^{u,1}},$$

this summation being a particular case of formula (XI.). The result agrees with $\mathrm{D}^u[1]$ formula (IX.), art. 3.

10. When the given elements are all finite in number, we may determine $\{u, \sigma\}$, by taking the sum of all the particular determinations that may be obtained pursuant to art. 9, by giving to z the successive values 0, 1, 2, 3, &c. If $u < s$, the upper limit of z is u, and the number of types to be formed is $[2u, u_1]$; which becomes $[2s, s_1]$, if $u=s$. If $u > s$, the upper limit of z is s; and the number of types to be formed is $[u+s, s_1]$. (See articles 4 and 5, Section I.) But, if the repetition is finite, some of these partitions may fail to yield combinations.

11. If the elements A, B, C, &c. represent different prime numbers, all the methods and theorems contained in this section will apply, *mutatis mutandis*, to the composite numbers of which those primes, or the powers of those primes, are divisors.

III. *On Permutations.*

1. Let the given elements be of s different kinds. We can determine in two known cases, by an explicit function of u, when the elements are taken u at a time, in how many different ways they can be permuted. The number of the permutations is denoted, when there is but one element of a kind, by $s^{u|-1}$; and when in all the kinds the elements are plural without limit, by s^u. When the plurality is finite, it is only in the particular case of all the elements being permuted at a time, that there is a known formula to express the number of their permutations.

2. Every combination constructed on a given type, $u=mv+m'v'+m''v''+$ &c., will generate the same number of permutations,

$$\frac{1^{u|1}}{[1^{v|1}]^m\,[1^{v'|1}]^{m'}\,[1^{v''|1}]^{m''}\,.\ \&c.} = \mathrm{P}.$$

Therefore, if the number of the different combinations which can be constructed out of the given elements in conformity with that type, is represented by Q, $\mathrm{Q} \times \mathrm{P}$ will be the number of the permutations corresponding to the type and to those elements. If the plurality be without limit,

$$\frac{s^{z|-1}}{1^{m|1}.\,1^{m'|1}.\,1^{m''|1}.\ \&c.} \times \mathrm{P}$$

will be the number of the permutations. If the given elements be finite in number, as in formulas (XIV.) and (XV.), the number of

the permutations corresponding to those elements and to the type, will be

$$\frac{t^{m}-1}{1^{m|1}} \cdot \frac{{}'t^{m'}-1}{1^{m'|1}} \cdot \frac{{}''t^{m''}-1}{1^{m''|1}} \text{ \&c.} \times \mathrm{P}.$$

Every different partition of u that may be formed within the limits pointed out in art. 10, Section II., will give rise to a similar product, $\mathrm{Q} \times \mathrm{P}$; and the sum of all these particular products, $\mathrm{S}[\mathrm{Q} \times \mathrm{P}]$, will show how many different permutations can be formed from the given elements, taken u at a time. The author illustrates this method of computing the number of permutations, by examples.

3. Let $\mathrm{P}\left\{\begin{matrix} u \\ s \end{matrix}\right\}$ denote how many different permutations can be formed when u elements are taken at a time out of s kinds; and $\mathrm{P}\{u, \sigma\}$ denote how many different permutations can be formed when u elements are taken at a time out of σ, a finite number of elements. If all the elements may be repeated without limit,

$$\left\{\begin{matrix} u \\ s \end{matrix}\right\} = \mathrm{D}^u\left[1^{u|1} . \varepsilon^{sx}\right] = s^u$$

$$= \mathrm{D}^u\left[1^{u|1}\left[1 + x + \frac{x^2}{1.2} + \ldots . \frac{x^u}{1^{u|1}} + \ldots .\right]^s\right].$$

Hence the author infers that, if the elements A are limited in number to α, while those of the other $(s-1)$ kinds are plural without limit,

$$\mathrm{P}\left\{\begin{matrix} u \\ s \end{matrix}\right\} = \mathrm{D}^u\left[1^{u|1}\, \varepsilon^{(s-1)x}\left[1 + x + \frac{x^2}{1.2} + \ldots + \frac{x^\alpha}{1^{\alpha|1}}\right]\right];$$

that if, moreover, the elements B are limited in number to β, while the other $(s-2)$ kinds are plural without limit,

$$\mathrm{P}\left\{\begin{matrix} u \\ s \end{matrix}\right\} = \mathrm{D}^u\left[1^{u|1}\, \varepsilon^{(s-2)x}\left[1 + x + \frac{x^2}{1.2} + \ldots \frac{x^\alpha}{1^{\alpha|1}}\right]\right.$$

$$\left.\left[1 + x + \frac{x^2}{1.2} + \ldots . \frac{x^\beta}{1^{\beta|1}}\right]\right];$$

and so on, until finally, if all the elements are finite in number, and the elements A, B, C, &c. are respectively limited, in point of number, to α, β, γ, &c.,

$$\left.\begin{aligned} \mathrm{P}\{u, \sigma\} = \mathrm{D}^u\Big[1^{u|1}\Big[1 + x + \ldots \frac{x^\alpha}{1^{\alpha|1}}\Big]\Big[1 + x + \ldots \frac{x^\beta}{1^{\beta|1}}\Big] \\ \Big[1 + x + \ldots \frac{x^\gamma}{1^{\gamma|1}}\Big]\Big]. \end{aligned}\right\}. \quad \text{(XVII.)}$$

4. Hence, if in all the s kinds the elements are dual, (XVII.) becomes

$$\left.\begin{array}{l} \mathrm{P}\{u,\sigma\}=\mathrm{D}^u\left[1^{u|1}\left[1+x+\frac{x}{2}\right]^s\right]= \\ \underset{\theta}{\mathrm{S}}\,{}^{\mathrm{I}\left(\frac{u}{2}\right)}_{0}\left[\frac{u^{2\theta|-1}\times s^{u-\theta|-1}}{2^{\theta|2}}\right]. \end{array}\right\} \quad . \quad \text{(XVIII.)}$$

This is the only addition which the author has been able to make to the cases wherein $\mathrm{P}\left[\begin{smallmatrix} u \\ s \end{smallmatrix}\right]$, or $\mathrm{P}\{u,\sigma\}$ is expressed by an explicit function of u, symmetrical in form.

Example. Let there be five kinds of elements, and two of each kind. Let $u=3$.

$$\mathrm{P}\{u,\sigma\}=\frac{1.5.4.3}{1}+\frac{3.2\times 5.4}{2}=120.$$

5. The author gives the following theorem, which is precisely analogous to that of art. 6, Sect. II., formula (XI.), in Combinations; viz.

$$\mathrm{P}\{u,\sigma\}=\underset{v}{\mathrm{S}}\,{}^{\tau}_{0}\left[\frac{u^{v|-1}}{1^{v|1}}\mathrm{P}\{v,\tau\}.\mathrm{P}\{u-v,\sigma-\tau\}\right]. \quad \text{(XIX.)}$$

6. By a mode of proof precisely analogous to that employed in art. 7, Sect. II., he shows that $\mathrm{P}\{\sigma-1,\sigma\}=\mathrm{P}\{\sigma,\sigma\}$; that is to say, that

$$\frac{1^{\sigma|1}}{1^{\alpha|1}.\,1^{\beta|1}.\,1^{\gamma|1}.\ \&c.}$$

denotes the number of permutations that can be formed with α elements A, β elements B, &c. (where $[\alpha+\beta+\gamma+\ \&c.]=\sigma$), as well when $\sigma-1$ elements, as when σ elements, are taken at a time.

Since correcting his paper for publication, the author has had his attention called to the work of Bézout on Elimination (4to. Paris, 1779, p. 469), as containing a formula similar in structure to that numbered VIII*. in the present abstract.

Bézout investigates the composition of a polynome function of s quantities, A, B, C, &c., consisting of terms which are of the form $\mathrm{A}^p\,\mathrm{B}^q\,\mathrm{C}^r$, and of every dimension from 0 to u inclusive. Let $[s]^u$ denote such a polynome, complete in all its terms, and $\mathrm{N}[s]^u$ the number of its terms. Then, 1st,

$$\mathrm{N}[s]^u=\frac{[u+1]^{s|1}}{1^{s|1}};$$

and 2nd, the number of the terms in $[s]^u$ which are not divisible by either A^α, or B^β, or C^γ, &c., he expresses by

$$\begin{aligned} &\mathrm{N}[s]^u-\mathrm{N}[s]^{u-\alpha}+\mathrm{N}[s]^{u-\alpha-\beta}-\ \&c. \\ &\quad -\mathrm{N}[s]^{u-\beta}+\ \&c. \\ &\quad -\ \&c. \end{aligned}$$

He also observes (p. 39) that when A^{α}, B^{β}, C^{γ}, &c. are the highest powers of A, B, C, &c. which a polynome, agreeing in other respects with $[s]^u$, contains, the terms of such incomplete polynome will agree in point of number with those terms in $[s]^u$ which are not divisible by either $A^{\alpha+1}$, or $B^{\beta+1}$, or $C^{\gamma+1}$, &c. The polynomes from which Bézout proposes to eliminate certain terms, contain terms of all dimensions from 0 to u inclusive. The terms which are to remain after the others have been eliminated, and which are enumerated by means of the condition, that they are not divisible by certain powers of A, or B, or C, &c., may be of all dimensions indiscriminately from 0 to u inclusive. Bézout's object is exclusively Elimination, and he makes no allusion to any other application of his formulæ.

The polynomes considered by the author, taken in their entirety, agree in their general structure with those considered by Bézout; but the nature of the author's inquiries led him to confine his attention to the composition of those particular terms in a polynome which were of the same dimension; and to seek to express the number of the terms, not of all dimensions indiscriminately, but of each particular dimension separately. To show how it has happened that researches, very different at their point of departure, have, as regards one point of investigation, ended in nearly similar formulæ, the author proceeds to deduce his formula (VIII*.) from the investigations of Bézout. Such a deduction, he conceives, might readily have been made by any one to whom it had occurred to make it; and the application of such a deduction, when once made, to problems in Combinations, would have been much too obvious to have remained long unnoticed.

Expressions of the form above considered are regarded by Bézout as of the nature of Differences; and the truth of this view of the subject may be shown in the following brief manner.

If $\phi(x)$ generates $\psi(u)$, $[1-x^{\alpha}]\phi(x)$ will generate $\psi(u)-\psi(u-\alpha)$, which we may denote by $\Delta_{\alpha}\psi(u)$. Consequently $[1-x^{\beta}][1-x^{\alpha}]$. $\phi(x)$, that is to say,

$$\left\{\begin{matrix} 1-x^{\alpha}+x^{\alpha+\beta} \\ -x \end{matrix}\right\}\phi(x)$$

will generate $\Delta^2_{\alpha,\beta}\psi(u)$; and so on; the independent variable, u, undergoing, not uniform, but variable decrements, α, β, γ, &c.

May 3, 1847.

On the Internal Pressure to which Rock Masses may be subjected, and its possible influence in the Production of the Laminated Structure. By W. Hopkins, M.A., F.R.S.

If a plane of indefinitely small extent pass through any proposed point in the interior of a continuous solid mass in a state of con-

straint, the resultant pressure or tension on this plane will vary with the angular position of the plane, and its direction will not, as in fluid masses, be generally perpendicular to the plane. There are, however, three angular positions in which the direction of the pressure does coincide with a perpendicular to the plane. These are called *principal directions*, and are at right angles to each other; the corresponding pressures are called *principal pressures*. In these particular positions of the plane there will be no *tangential* action upon it; but generally the whole pressure or tension may be resolved into two parts, of which one is *normal* and the other *tangential*. In certain positions of the plane these forces assume their maximum or minimum values. The normal action is a maximum, when a perpendicular to the plane coincides with one of the three principal directions; and a minimum, when it coincides with another, the third of those directions, not corresponding either to a maximum or minimum value. These conclusions have been established by Poisson, Cauchy and others. In this paper the author has investigated the positions of the small plane, when the *tangential force* upon it is a maximum. There are two of these positions perpendicular to each other, in each of which the plane passes through that principal direction which does not correspond to either the maximum or minimum value of the normal force, and bisects the corresponding right angle between the other two principal directions—those of the maximum and minimum normal forces. Having established the relative positions of the planes of greatest normal and of greatest tangential action, the author proceeds to examine how far the evidence afforded by the distorted forms of organic remains may justify the conclusion that these forces have had an influence in determining the position of the planes of cleavage in the rocks containing those remains.

Conceive one stratified bed placed on another, and acted on by forces tending to give the upper a small sliding motion along the surface of the lower one. A considerable *tangential force* will be called into action between the beds; and if any object be placed between them, its lower part will be pushed in one direction by the action of the lower bed, while its upper part will be equally pushed in the opposite direction by the action of the upper bed, and thus the object will be *twisted* from its original form. For example, suppose the object be an equilateral shell lying between the two beds, with the plane of junction of the two valves parallel to the surfaces of the beds, and suppose the median line of either valve to be perpendicular to the direction in which the one bed tends to move along the other. The shell in its distorted form will no longer be equilateral; one half of each shell will be crumpled into a smaller space, while the other half will be extended into greater breadth; so that if there be longitudinal folds on the valve, those on the former half will be pressed together, and those on the latter will be dilated into greater breadth. An exactly similar effect will be produced on both shells; but the compressed half of one will be opposite to the dilated half of the other.

Again, suppose the beds to be acted on by forces tending to com-

press them equally in a direction parallel to their surfaces. The shell will then be *compressed* in the same direction, so that, generally, the ratio of the length to the breadth of the shell will be altered, but without that *twisting* which will characterize the distorted form in the former case. In the case of this paragraph, the direction of compression will coincide with what has been above termed a *principal direction*, and it will also be that of *maximum normal pressure*. In the previous case, the common surface of the two beds will be the plane of *maximum tangential action*.

If, then, in any stratified mass, we observe the organic remains to be regularly distorted, and *twisted* from their original forms, as above described, we may conclude that the planes of stratification have nearly coincided with those of *maximum tangential action*; but if, on the contrary, the distortion consists only in compression of the shells in a given direction along the surface of the bed where they are found, we may conclude that the direction of *maximum normal pressure* has nearly coincided with this direction of compression, and was consequently parallel to the planes of stratification. The masses in which distorted remains have been found, are generally those which have been much disturbed. The disturbing forces are those to which the distortions are to be referred; and it may be remarked, that in such cases the directions of maximum and minimum pressure at any point would probably lie in a plane perpendicular to the strike of the elevated beds, and that consequently the planes of maximum tangential action, which bisect the angles between those directions, will have approximately the same strike as the beds themselves.

The bearing of these conclusions on the question of laminated structure is easily seen. Suppose the planes of lamination are observed to be nearly coincident with those of stratification, and that the distortion of the organic remains consists in their being *twisted* from their primitive forms. Then, if the position of the planes of lamination has been due to the internal pressures to which the mass has been subjected, it is to *tangential action*, and not to *direct pressure*, that the effect is attributable. Again, if the planes of lamination have nearly the same strike as the beds, and are inclined to them at an angle of about 45°, while the organic remains have been distorted only by *direct compression*, the planes of lamination must in this case also have coincided with those of maximum tangential action, and we shall have the same conclusion as in the former case. The direction of compression of the organic forms ought, according to this view, to be perpendicular to the intersections of the planes of lamination and those of stratification.

Mr. Sharpe, in a paper recently published in the Journal of the Geological Society, has stated nearly all the evidence hitherto collected on this subject; and it appears that the organic bodies are most *twisted* from their original forms in those cases in which the planes of lamination coincide most nearly with those of stratification, and that they have generally suffered most *direct compression* without twisting in those cases in which the planes of lamination are inclined to those of stratification at an angle of 40° or 50°. We must

therefore conclude, according to the last paragraph, that *the planes of lamination approximately coincide with those which were formerly the planes of greatest tangential action.*

The author does not regard this mechanical action as the probable primary cause of the laminated structure, but rather as a secondary cause, which may have had its influence in determining the positions of the planes of lamination. He trusts that further evidence will be collected on the subject.

May 17, 1847.

On the Symbolical Equation of Vibratory Motion of an Elastic Medium, whether Crystallized or Uncrystallized. By the Rev. M. O'Brien, late Fellow of Caius College, Professor of Natural Philosophy and Astronomy in King's College, London.

The object of the author in this paper is twofold: *first*, to show that the equations of vibratory motion of a crystallized or uncrystallized medium may be obtained in their most general form, and very simply, without making any assumption as to the nature of the molecular forces; and *secondly*, to exemplify the use of the symbolical method and notation explained in two papers read before the Society during the present academical year.

First, with regard to the method of obtaining the equations of vibratory motion.

This method consists in representing the *disarrangement* (or state of relative displacement) of the medium in the vicinity of the point xyz by the equation

$$\delta v = \frac{dv}{dx}\delta x + \frac{dv}{dy}\delta y + \frac{dv}{dz}\delta z + \frac{1}{2}\frac{d^2v}{dx^2}\delta x^2 + \frac{d^2v}{dxdy}\delta x\delta y + \&c. - \&c.$$

(where $v = \xi\alpha + \eta\beta + \zeta\gamma$, $\xi\eta\zeta$ denoting, as usual, the displacements at the point xyz, and $\alpha\beta\gamma$ being the *direction units* of the three coordinate axes), and in finding the *whole* force brought into play at the point xyz (in consequence of this disarrangement) by the *symbolical addition* of the different forces brought into play by the several terms of δv, *each considered separately*. It is easy to see that these different forces may be found with great facility, without assuming anything respecting the constitution of the medium more than this, that it possesses *direct* and *lateral elasticity*. By *direct elasticity* we mean that elasticity in virtue of which *direct* or *normal* vibrations take place; and by *lateral*, that in virtue of which *lateral* or *transverse* vibrations take place.

The forces due to the several terms of δv are obtained by means of the following simple considerations.

Let AB be any line in a perfectly uniform medium, and conceive the medium to be divided into elementary slices by planes perpendicular to AB; let OM$(=x)$ be the distance of any slice PP′ from any particular point O of AB, and suppose this slice to suffer a dis-

placement equal to $\frac{1}{2}cx^2$ (c being a constant) *in* the direction OAB, and the other slices to be similarly displaced. Then it is evident that the medium suffers by these displacements a uniformly increasing *expansion* in the direction OB, and a uniformly increasing condensation in the direction OA; the rate of increase both of the expansion and condensation being c. Now in all known substances, whether solid, fluid, or gaseous, a disarrangement of this kind would bring into play on the slice O a force along the line AB proportional to the rate of increase c, *i. e.* a force Ac, A being a constant depending upon what we may call the *direct elasticity* of the substance.

Again, suppose that the slice PP′ receives a displacement $\frac{1}{2}cx^2$ in the direction OC *perpendicular* to AB, and the other slices similar displacements. Then the line AB will become curved into a parabola A′OB′, and all the lines of the medium parallel to AB will be similarly curved, the radius of curvature being equal to $\frac{1}{c}$ and perpendicular to AB. Now in all known substances* a disarrangement of this kind would bring into play upon the slice O a force in the direction OC proportional to the curvature c, *i. e.* a force Bc depending upon what we may call the *lateral elasticity* of the substance.

Lastly, suppose that MP$=y$, and that the point P of the medium receives a displacement cxy parallel to AB, and the other points similar displacements. Then the slice PP′ will, in consequence of this kind of displacement, turn through an angle $\tan^{-1}(cx)$ into the dotted position, and the other slices will suffer similar rotations, those on the other side of O, such as QQ′, turning the opposite way. Now it is easy to see that a disarrangement of this kind produces a uniformly increasing expansion in the direction OC, and a uniformly increasing condensation in the direction OC′, the rate of increase both of the expansion and condensation being c. But the expansion and condensation here described are quite different from that previously noticed; since it is produced, not by displacements parallel to C′C, but by *lateral* displacements, *i. e. perpendicular* to C′C. On this account all that we can assert without further investigation is, that the force brought into play upon an element at O by this disarrangement acts along the line C′C, and is proportional to c, *i. e.* equal to Cc, where C is some constant evidently depending in some way both upon the *direct* and *lateral* elasticity of the medium.

There is however a very simple way of finding the precise value of the force brought into play by a disarrangement of this kind; for if we turn the axes of x and y in the plane of the paper through an angle of 45°, it appears that this disarrangement is nothing but a combination of the two kinds of disarrangement previously noticed; and from this it immediately follows, in the case of an uncrystallized medium, that the force brought into play at O is (A—B)c; in other

* Fluids and gases possess lateral elasticity as well as solids, only in a comparatively feeble degree.

words, the coefficient C, which must be multiplied into c, in order to give the force brought into play by the disarrangement cxy, is equal to the coefficient of direct elasticity (A) minus the coefficient of lateral elasticity (B).

In the case of a crystallized medium, it may be shown that *six relations*, corresponding to the relation $C = A - B$, are most probably true, and are *essential* to Fresnel's theory of transverse vibrations; that is to say, the medium is capable of propagating waves of transverse vibrations if these six conditions hold, but otherwise it is not.

In employing the above considerations to determine the equations of vibratory motion, the directions AB and C′C are always taken so as to coincide with some two of the three coordinate axes; and it is this circumstance that makes the method peculiarly applicable to crystallized media. Indeed, if it were necessary to take the lines AB and C′C in any directions but those of the axes of symmetry, the above considerations would not apply without considerable modification.

The equations of vibratory motion obtained by this method for an uncrystallized medium, are the well-known equations involving the two constants A and B. The equations obtained for a crystallized medium are perfectly free from any restriction of any kind, are applicable to all kinds of substance, whether we suppose its structure to be analogous to that of a solid fluid or gas, and hold for all kinds of disarrangement, whether consisting of normal or transverse displacements, or both.

When we introduce the six relations between the constants above alluded to, and moreover assume that the vibrations constituting a polarized ray are *in* the plane of polarization, we arrive at Professor MacCullagh's equations*. If, on the contrary, we suppose the vibrations to be *perpendicular* to the plane of polarization, we arrive at equations which agree exactly with Fresnel's theory in every particular†.

If we introduce these six relations into the equations for crystallized media deduced from M. Cauchy's hypothesis, that the molecular forces act along the lines joining the different particles of the medium, it will be found that these equations are immediately reduced to the equations for an uncrystallized medium. From this it follows that M. Cauchy's hypothesis cannot be applied to any but uncrystallized media. In fact, it may be easily proved that if the equations derived from this hypothesis be true, a crystallized medium is incapable of propagating transverse vibrations.

Secondly, respecting the use of the symbolical method and notation above alluded to.

The application of the *symbolical method and notation* to the subject of vibratory motion is very remarkable, and leads to equations of great simplicity. In the case of an uncrystallized medium, the three

* Given in a paper read to the Royal Irish Academy, Dec. 9, 1839, page 14.

† On this subject see a paper by the late Mr. Greene in the seventh volume of the Cambridge Transactions, p. 121.

ordinary equations of motion are included in the single symbolication equation

$$\frac{d^2v}{dt^2}=B\left\{\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right\}v+(A-B)\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\left(\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}\right).$$

If we employ the notation $\Delta u'.u$, and assume the symbol $\mathfrak{D}$ to represent the operation

$$\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz},$$

the equation of motion becomes

$$\frac{d^2v}{dt^2}=B(\Delta\mathfrak{D}.\mathfrak{D})v+(A-B)+\Delta\mathfrak{D}.v;$$

or, by using the notation $Du'.u$ also, it may be put in the form

$$\frac{d^2v}{dt^2}=\{A\mathfrak{D}\Delta\mathfrak{D}.-B(D\mathfrak{D}.)^2\}v.$$

The symbol $\mathfrak{D}$ written before any quantity U which is a function of xyz, has a very remarkable signification; the *direction unit* of the symbol $\mathfrak{D}U$ is that direction *perpendicular* to which there is no variation of U at the point xyz, and the *numerical magnitude* of $\mathfrak{D}U$ is the *rate of variation* of U, when we pass from point to point *in that direction.*

The symbols $\Delta\mathfrak{D}.v$ and $D\mathfrak{D}.v$ have also remarkable significations. $\Delta\mathfrak{D}.v$ is a numerical quantity representing the *degree of expansion*, or what is called the *rarefaction* of the medium at the point xyz. $D\mathfrak{D}.v$ represents, in magnitude, the degree of *lateral disarrangement* of the medium at the point xyz, and, in direction, the *axis* about which that displacement takes place.

These two symbols may be found separately by the integration of an equation of the form

$$\frac{d^2U}{dt^2}=C\left(\frac{d^2U}{dx^2}+\frac{d^2U}{dy^2}+\frac{d^2U}{dz^2}\right).$$

When the six conditions above alluded to are introduced, the equation of motion for a crystallized medium becomes

$$\frac{d^2v}{dt^2}=\left(A_1\alpha\frac{d}{dx}+A_2\beta\frac{d}{dy}+A_3\gamma\frac{d}{dz}\right)\Delta\mathfrak{D}.v$$
$$+D\mathfrak{D}.\left\{\left(B_2\frac{d\eta}{dz}-B'_3\frac{d\zeta}{dy}\right)\alpha+\left(B_3\frac{d\zeta}{dx}-B'_1\frac{d\xi}{dz}\right)\beta+\right.$$
$$\left.\left(B_1\frac{d\xi}{dy}-B'_2\frac{d\eta}{dx}\right)\gamma\right\},$$

where $A_1\ A_2\ A_3$ are the three coefficients of *direct* elasticity with reference to the three axes of symmetry, and $B_1\ B_1'\ B_2\ B_2'\ B_3\ B_3'$ the six coefficients of *lateral* elasticity with reference to the same axes.

If the vibrations be transverse, this equation is reducible to the form

$$\frac{d^2v}{dt^2}=-(D\mathfrak{D}.)^2(a^2\xi\alpha+b^2\eta\beta+c^2\zeta\gamma)$$
$$=-(D\mathfrak{D}.)^2(a^2\alpha\Delta\alpha+b^2\beta\Delta\beta+c^2\gamma\Delta\gamma)v,$$

assuming the vibrations of a polarized ray to be *perpendicular* to the plane of polarization.

The well-known condition that a plane polarized ray may be transmissible without subdivision, and the velocity of propagation may be immediately deduced from this equation.

If we assume the vibrations of a polarized ray to be *in* the plane of polarization, the equation becomes

$$\frac{d^2v}{dt^2} = -\mathrm{D}\mathfrak{D}.(a^2\alpha\Delta\alpha + b^2\beta\Delta\beta + c^2\gamma\Delta\gamma)\mathrm{D}\mathfrak{D}\ v.$$

This includes Professor MacCullagh's three equations.

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December 6, 1847.

On the Critical Values of the sums of Periodic Series. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

There are a great many problems in heat, fluid motion, &c., the solution of which requires the development of an arbitrary function of x, $f(x)$, between certain limits as o and a of x, by means of functions of known form. The form of the expansion is determined, at least in part, by the conditions to be satisfied at the limits; and it is usually considered that these conditions are satisfied by adopting the form of expansion to which they lead. Thus, if the problem requires that $f(o)$ and $f(a)$ vanish, it is considered that this condition is satisfied by developing $f(x)$ in a series of sines of $\frac{\pi x}{a}$ and its multiples. But since an arbitrary function admits of expansion in such a series, the expanded function is not restricted to vanish at the limits o and a. It becomes then a question, how shall we know when the expanded function does really vanish at the limits, and if it does not, how are such expansions to be treated, and are they of any practical importance?

In considering the logic of such developments, the author was led to perceive in what manner the evanescence of $f(x)$ at the limits can be ascertained, or else the values of $f(o)$ and $f(a)$ obtained, from the development itself, even when the series cannot be summed, by examining the coefficient of $\sin \frac{n\pi x}{a}$ in the nth term. In a similar manner the discontinuity of $f(x)$ or any of its derivatives may be ascertained, and the amount of the sudden change of the function determined. In such cases the expansions of the derivatives of $f(x)$ cannot be obtained by differentiating under the sign of summation, but are given by formulæ which the author has considered.

The most important case in considering a series of sines, is that in which $f(x)$ is continuous; but $f(o)$ and $f(a)$, instead of being equal to zero, are given quantities, and the coefficients in the expansion are indeterminate. In this case the coefficients in the expansions of $f'(x)$ and $f''(x)$ contain, in addition to the indeterminate coefficients which enter into the expansion of $f(x)$, the given quantities $f(o)$ and

$f(a)$. Thus the expansion in a series of sines is useful, not only when $f(o)$ and $f(a)$ vanish, but also when they are given quantities. In the same way the expansion of $f(x)$ in a series of cosines is useful when $f'(o)$ and $f'(a)$ are given, as well as when they vanish. Thus, to take an example, the permanent temperature in a rectangular parallelepiped, when the temperatures of the faces are any arbitrary functions of the co-ordinates, can be expressed in a double series of sines involving *any two* of the three co-ordinates.

The author has only considered a series of sines and a series of cosines, with the corresponding integrals; but the methods which he has employed are of very general application. The comparison of different expressions of the same function of two or more independent variables often leads to very remarkable formulæ. The development of arbitrary functions in the way considered by the author is, however, not only curious but useful; for the expressions thus obtained are often much better adapted to numerical computation than those which would be obtained by the developments usually employed.

In connexion with these investigations, the author was led to consider the discontinuity of the sums of infinite series, or of the values of integrals between infinite limits, which sometimes takes place even when the series or integral remains convergent, and the general term of the series, or the quantity under the integral sine, is a continuous function of some quantity which is regarded as variable. The author has shown that in all such cases the convergency of the series or integral becomes infinitely slow.

The problem of determining the potential due to a given electrical point within a hollow conducting rectangular parallelepiped, and to the electricity included on the surface, is solved by a method which leads very readily to the result. The author thinks that a similar method may sometimes be advantageously employed in other questions. The electricity is first supposed to be diffused over a finite space: this allows of the expansion of the potential V in a triple series of sines. Instead of the equation $\nabla V = 0$, where ∇ means the same as

$$\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2},$$

it becomes of course necessary to employ the equation $\nabla V = -4\pi\rho$. The solution having been obtained, the electricity may now be supposed to be condensed into a point, and one of the summations may be effected. The potential is thus expressed in a double series, which appears to be the simplest form that it admits of.

May 8, 1848.

Supplement to a paper "On the Intensity of Light in the neighbourhood of a Caustic." By G. B. Airy, Esq., Astronomer Royal.

The author, after referring to the paper printed in a former volume of the Society's Memoirs, in which he has shown that the expression for the intensity depends on the integral $\int_w \cos(w^3 - mw)$ between the limits $w=0$, $w=$ infinity, where m is proportional to the distance of the point at which the intensity is required, from the geometrical caustic, and in which he has calculated by quadratures the value of the definite integral for different values of m as far as $m=\pm 4{\cdot}0$, states that he was induced to have recourse to the method of quadratures only because every expansion which he attempted made it necessary to rely (for some of the terms) upon definite integrals equivalent to the integral $\int_\theta \cos\theta$ from $\theta=0$ to $\theta=$ infinity, and that he was not satisfied with the reasoning upon which some mathematicians had given a determinate value to that integral. Professor De Morgan, however, who felt no doubts upon it, had furnished him with a series proceeding by ascending powers of m, and had also explained in detail (in a letter embodied in this paper) his views on the evidence for the value of the series, and on the method of determining it. From this series, the values of the definite integral are computed for all the values of m for which the computation had been made by quadratures, and the result is that the two sets of computed numbers are entirely accordant. The computations are also extended to the limit $m=\pm 5{\cdot}6$, which is the greatest value to which it is possible to extend the calculations by the use of 10-figure logarithms.

May 22, 1848.

Some Remarks on the Theory of Matter. By R. L. Ellis, Esq., M.A., Fellow of Trinity College, Cambridge.

The question to which these remarks principally relate is this: Can all phænomena, *e. g.* those of chemistry, be explained mechanically? The writer, assuming that this question is to be answered negatively, endeavours to determine what principles of causation, beside mechanical force, may be introduced into physical theories, consistently with the doctrine that the secondary qualities of bodies are to be explained by means of the primary. His conclusion is, that we are at liberty, in constructing an hypothesis as to the mode of action of matter on matter, to introduce a new principle of causation (which he calls $(\text{force})^2$), bearing the same relation to force that force does to velocity; and further, that following the analogy here suggested, we may introduce an indefinite number of such principles, viz. $(\text{force})^3 \ldots (\text{force})^n$, &c., all essentially distinct from one another, and from those previously recognised.

But, on the other hand, he conceives that it is necessary to reject any modification of *qualitative* action; and that consequently physical science, though it may cease to be wholly *mechanical*, will yet always

continue to be *cinematical*, in the largest sense of which this word (so far as relates to local motion) can admit.

June 5, 1848.

Methods of Integrating Partial Differential Equations. By Prof. De Morgan.

This paper contains a sketch of two distinct methods. In the first (x, y, z, p, q, r, s, t, having their usual significations) the given equation is supposed to be of the form $\phi(x, y, p, q)=0$, and this is made the result of elimination between two equations involving a new variable v. From these two, and their four differentials of the first order, p, q, r, s, t are eliminated, and an equation of the first order results between x, y, v. This last equation is often more manageable than the original one.

The process is rendered very simple when the given equation can be reduced to depend on two of the form

$$p=\phi(x, y, v) \quad q=\psi(x, y, v).$$

The second method was completed, Mr. De Morgan states, and out of his hands for transmission to the Society, when he discovered that Mouge had communicated it to the Institute, by which body it was never published. But M. Chasles found it among the manuscripts of the Institute, and stated it a few years ago in one of the notes to his *Aperçu Historique . . . . des Méthodes en Géométrie*. Its occurrence in the voluminous additions made to a work which itself treats only of geometry, seems to have prevented it from becoming known to any writer on the differential calculus. Certain particular cases appear in the writings of Legendre and Lacroix.

Let the equation be $\phi(x, y, z, p, q, r, s, t)=0$. Change x into p, y into q, z into $px+qy-z$, p into x, q into y, r into $\frac{t}{rt-s^2}$, s into $\frac{-s}{rt-s^2}$, t into $\frac{r}{rt-s^2}$. If the equation thus resulting can be integrated, let its solution be $Z=\psi(X, Y)$. Then the solution of the original equation can be obtained by eliminating X, Y, Z from

$$Z=\psi(X, Y) \quad x=\frac{dZ}{dX} \quad y=\frac{dZ}{dY} \quad z=xX+yY-Z.$$

In both methods the most effective mode of proceeding is to find what Lagrange calls a *primary solution*, containing two arbitrary constants, and then to use that primary solution.

On some new Fossil Fish of the Carboniferous Period. By Frederic M'Coy, M.G.S., N.H.S.D.

The author having premised that the species of fish of the carboniferous limestone enumerated in the third volume of the *Poissons Fossiles* of M. Agassiz are for the most part still unpublished, being without definitions or figures, states that through the kindness of

Capt. Jones, R.N., M.P., &c. he was enabled to study the original specimens of twenty-eight out of the thirty unpublished species from Armagh in M. Agassiz's list, and is therefore certain of the species described by him being so far distinct from those alluded to. The greater number of the examples here described are in the cabinets of the University of Cambridge (principally collected by the Rev. W. Stokes, of Caius College), and of Captain Jones; a few from the lower carboniferous shales of Ireland are only known in that of Mr. Griffith of Dublin. The descriptions are accompanied by drawings of all the species of the natural size, and illustrations of the microscopic structures; and acknowledgements are made of the kind co-operation of the Rev. Prof. Clark and Mr. Anthony of Caius College, Cambridge, in this part of the investigation, by allowing the use of their large microscopes, and assisting to prepare the transparent fragments for examination.

Twelve new genera are proposed:—1st. *Isodus*, for a fish of the yellow sandstone, having very numerous teeth with a simple conical pulp-cavity in their upper part, which divides into branches below as in *Rhizodus* (Owen); but the section is circular, and the teeth are all equal in size. 2nd. *Centrodus*, for curved conical teeth with a wide simple pulp-cavity, reducing the base to a sharp edge, and having not only the form but the microscopic structure of a reptile tooth, that is, from the simple pulp-cavity minute calcigerous tubes radiate to the circumference, terminating near the surface in a layer of small calcigerous cells, covered by a layer of true glass-like enamel, presenting no trace of structure with a power of 300 diameters, and quite distinct from that dense modification of *dentine*, which, forming the polished surface of most fish-teeth, has been confounded with true enamel, but which it is here proposed to call *ganoine* in future descriptions. 3rd. *Colonodus*, for very long simple teeth with simple pulp-cavity, and their sides indented by transverse wrinkles. 4th. *Osteoplax*, for large, flat, polygonal dermal plates, minutely wrinkled on the surface, and allied to *Psammosteus* (Ag.) of the Old Red Sandstone; but while the latter plates are composed of horizontal layers of large cells, the present genus has a very singular microscopic structure, being traversed by vertical branched (Haversian?) canals terminating in the pores of the surface; and in the intervening blastema are numerous oval Purkinian cells, the radiating tubuli of which do not anastomose. 5th. *Erismacanthus*, for a singular Ichthyodorulite not uncommon in the Armagh limestone, which, arising from a large compressed base, branches into two portions, one long anterior closely tuberculated prop-like portion, and another extruding backwards, short, and resembling a small *Ctenacanthus*, but with smooth ridges. 6th. *Platycanthus*, for small spines, extremely wide and compressed, resembling small Oracanths, but arched and with posterior rows of teeth. 7th. *Dipriacanthus*, for small, curved dorsal spines, which have two rows of denticles pointing downwards on the posterior face, and two rows pointing upwards on the anterior face, reminding us of the recent *Pimelodus* and *Synodontus* of the Nile. 8th. *Polyrhizodus*, an extraordinary genus of Psammodontoid teeth

not uncommon in the Armagh limestone, having the root divided into numerous fang-like lobes, as in a mammalian tooth. 9th. *Glossodus*, for certain tongue-shaped teeth allied to *Helodus* (Ag.). 10th. *Climaxodus*, for some palates allied to *Pœcilodus* (Ag.), but instead of being transversely trigonal and obliquely ridged, they are equilateral, and have the ridges transverse and parallel (like a flight of steps). 11th. *Chirodus*, for little hand-shaped teeth allied to the *Ceratodi*, but distinguished by the thumb-like lobe projecting from the middle of the long side, and which would prevent the union of the teeth in pairs in the mouth, in the manner of *Ceratodus*. 12th. *Petrodus*, small conical ridged teeth resembling limpets, common in the Derbyshire limestone, but presenting, of all known fossil fish, the nearest approach to the microscopic structure of the recent *Cestracion*. It is also proposed to divide the genus *Holoptychius* of M. Agassiz; and instead of considering it and *Rhizodus* of Owen as synonymous, to limit the latter to those great teeth with an elliptical section so common in some parts of the Carboniferous series, accompanied by large, thin, quadrate scales, marked with concentric lines of growth, and having a fine cancellated structure internally, the *Holoptychius Hibberti* (Ag.) *Rhizodus ferox*, (Owen) and *H. Portlocki* (Ag.) being the types; thus retaining the name *Holoptychius* for those fish so abundant in the Old Red Sandstone with thick, bony, ovate, longitudinally wrinkled scales, and minute teeth with a circular section, having the *H. nobilissimus*, *H. giganteus*, &c. as the type.

The number of new species described and figured in this paper is forty-one, of which several belong to genera not previously known in rocks of the carboniferous period, many showing a strong affinity to the Devonian type of form. Thus we have two species of *Psammosteus*, one of *Chelyophorus*, one (doubtful) of *Coccosteus*, one of *Asterolepis*, two of *Homacanthus*, and one of *Cosmacanthus*, genera hitherto only found in the Old Red Sandstone.

On an Absolute Thermometric Scale founded on Carnot's Theory of the Motive Power of Heat*, and calculated from Regnault's observations†. By Prof. W. Thomson, Fellow of St. Peter's College.

The determination of temperature has long been recognized as a problem of the greatest importance in physical science. It has accordingly been made a subject of most careful attention, and, especially in late years, of very elaborate and refined experimental re-

* Published in 1824 in a work entitled *Réflexions sur la Puissance Motrice du Feu*, by M. S. Carnot. Having never met with the original work, it is only through a paper by M. Clapeyron, on the same subject, published in the *Journal de l'Ecole Polytechnique*, vol. xiv. 1834, and translated in the first volume of Taylor's Scientific Memoirs, that the author has become acquainted with Carnot's theory.—W. T.

† An account of the first part of a series of researches undertaken by M. Regnault by order of the French Government, for ascertaining the various physical data of importance in the Theory of the Steam-Engine, is just published in the *Mémoires de l'Institut*, of which it constitutes the twenty-first volume (1847). The second part of the researches has not yet been published.

searches* ; and we are thus at present in possession of as complete a practical solution of the problem as can be desired, even for the most accurate investigations. The theory of thermometry is however as yet far from being in so satisfactory a state. The principle to be followed in constructing a thermometric scale might at first sight seem to be obvious, as it might appear that a perfect thermometer would indicate equal additions of heat, as corresponding to equal elevations of temperature, estimated by the numbered divisions of its scale. It is however now recognized (from the variations in the specific heats of bodies) as an experimentally demonstrated fact that thermometry under this condition is impossible, and we are left without any principle on which to found an absolute thermometric scale.

Next in importance to the primary establishment of an absolute scale, independently of the properties of any particular kind of matter, is the fixing upon an arbitrary system of thermometry, according to which results of observations made by different experimenters, in various positions and circumstances, may be exactly compared. This object is very fully attained by means of thermometers constructed and graduated according to the clearly defined methods adopted by the best instrument-makers of the present day, when the rigorous experimental processes which have been indicated, especially by Regnault, for interpreting their indications in a comparable way, are followed. The particular kind of thermometer which is least liable to uncertain variations of any kind is that founded on the expansion of air, and this is therefore generally adopted as the standard for the comparison of thermometers of all constructions. Hence the scale which is at present employed for estimating temperature is that of the air-thermometer ; and in accurate researches care is always taken to reduce to this scale the indications of the instrument actually used, whatever may be its specific construction and graduation.

The principle according to which the scale of the air-thermometer is graduated, is simply that equal absolute expansions of the mass of air or gas in the instrument, under a constant pressure, shall indicate equal differences of the numbers on the scale ; the length of a "degree" being determined by allowing a given number for the interval between the freezing- and the boiling-points. Now it is found by Regnault that various thermometers, constructed with air under different pressures, or with different gases, give indications which coincide so closely, that, unless when certain gases, such as sulphurous acid, which approach the physical condition of vapours at saturation, are made use of, the variations are inappreciable†. This remarkable circumstance enhances very much the practical value of the air-thermometer ; but still a rigorous standard can only be defined by

* A very important section of Regnault's work is devoted to this object.

† Regnault, *Relation des Expériences*, &c., Fourth Memoir, First Part. The differences, it is remarked by Regnault, would be much more sensible if the graduation were effected on the supposition that the coefficients of expansion of the different gases are equal, instead of being founded on the principle laid down in the text, according to which the freezing- and boiling-points are experimentally determined for each thermometer.

fixing upon a certain gas at a determinate pressure, as the thermometric substance. Although we have thus a strict principle for constructing a *definite* system for the estimation of temperature, yet as reference is essentially made to a specific body as the standard thermometric substance, we cannot consider that we have arrived at an *absolute* scale, and we can only regard, in strictness, the scale actually adopted as *an arbitrary series of numbered points of reference sufficiently close for the requirements of practical thermometry.*

In the present state of physical science, therefore, a question of extreme interest arises: *Is there any principle on which an absolute thermometric scale can be founded?* It appears to me that Carnot's theory of the motive power of heat enables us to give an affirmative answer.

The relation between motive power and heat, as established by Carnot, is such that *quantities of heat*, and *intervals of temperature*, are involved as the sole elements in the expression for the amount of mechanical effect to be obtained through the agency of heat; and since we have, independently, a definite system for the measurement of quantities of heat, we are thus furnished with a measure for intervals according to which absolute differences of temperature may be estimated. To make this intelligible, a few words in explanation of Carnot's theory must be given; but for a full account of this most valuable contribution to physical science, the reader is referred to either of the works mentioned above (the original treatise by Carnot, and Clapeyron's paper on the same subject).

In the present state of science no operation is known by which heat can be absorbed, without either elevating the temperature of matter, or becoming latent and producing some alteration in the physical condition of the body into which it is absorbed; and the conversion of heat (or *caloric*) into mechanical effect is probably impossible*, certainly undiscovered. In actual engines for obtaining mechanical effect through the agency of heat, we must consequently look for the source of power, not in any absorption and conversion, but merely in a transmission of heat. Now Carnot, starting from universally acknowledged physical principles, demonstrates that it is by the *letting down* of heat from a hot body to a cold body, through the medium of an engine (a steam-engine, or an air-engine for instance), that mechanical effect is to be obtained; and conversely, he proves that the same amount of heat may, by the expenditure of an equal amount of labouring force, be *raised* from the cold to the hot body (the engine being in this case *worked backwards*); just as mechanical

* This opinion seems to be nearly universally held by those who have written on the subject. A contrary opinion however has been advocated by Mr. Joule of Manchester; some very remarkable discoveries which he has made with reference to the *generation* of heat by the friction of fluids in motion, and some known experiments with magneto-electric machines, seeming to indicate an actual conversion of mechanical effect into caloric. No experiment however is adduced in which the converse operation is exhibited; but it must be confessed that as yet much is involved in mystery with reference to these fundamental questions of natural philosophy.

effect may be obtained by the descent of water let down by a water-wheel, and by spending labouring force in turning the wheel backwards, or in working a pump, water may be elevated to a higher level. The amount of mechanical effect to be obtained by the transmission of a given quantity of heat, through the medium of any kind of engine in which the œconomy is perfect, will depend, as Carnot demonstrates, not on the specific nature of the substance employed as the medium of transmission of heat in the engine, but solely on the interval between the temperatures of the two bodies between which the heat is transferred.

Carnot examines in detail the ideal construction of an air-engine and of a steam-engine, in which, besides the condition of perfect œconomy being satisfied, the machine is so arranged, that at the close of a complete operation the substance (air in one case and water in the other) employed is restored to precisely the same physical condition as at the commencement. He thus shows on what elements, capable of experimental determination, either with reference to air, or with reference to a liquid and its vapour, the absolute amount of mechanical effect due to the transmission of a unit of heat from a hot body to a cold body, through any given interval of the thermometric scale, may be ascertained. In M. Clapeyron's paper various experimental data, confessedly very imperfect, are brought forward, and the amounts of mechanical effect due to a unit of heat descending a degree of the air-thermometer, in various parts of the scale, are calculated from them, according to Carnot's expressions. The results so obtained indicate very decidedly, that what we may with much propriety call *the value of a degree* (estimated by the mechanical effect to be obtained from the descent of a unit of heat through it) of the air-thermometer depends on the part of the scale in which it is taken, being less for high than for low temperatures*.

The characteristic property of the scale which I now propose is, that all degrees have the same value; that is, that a unit of heat descending from a body A at the temperature T° of this scale, to a body B at the temperature $(T-1)^\circ$, would give out the same mechanical effect, whatever be the number T. This may justly be termed an absolute scale, since its characteristic is quite independent of the physical properties of any specific substance.

To compare this scale with that of the air-thermometer, the *values* (according to the principle of estimation stated above) of degrees of the air-thermometer must be known. Now an expression, obtained by Carnot from the consideration of his ideal steam-engine, enables

* This is what we might anticipate, when we reflect that infinite cold must correspond to a finite number of degrees of the air-thermometer below zero; since, if we push the strict principle of graduation, stated above, sufficiently far, we should arrive at a point corresponding to the volume of air being reduced to nothing, which would be marked as -273° $\left(-\frac{100}{\cdot 366}\right.$, if ·366 be the coefficient of expansion) of the scale; and therefore -273° of the air-thermometer is a point which cannot be reached at any finite temperature, however low.

us to calculate these values, when the latent heat of a given volume and the pressure of saturated vapour at any temperature are experimentally determined. The determination of these elements is the principal object of Regnault's great work, already referred to, but at present his researches are not complete. In the first part, which alone has been as yet published, the latent heats of a given *weight*, and the pressures of saturated vapour at all temperatures between 0° and 230° (Cent. of the air-thermometer), have been ascertained; but it would be necessary in addition to know the densities of saturated vapour at different temperatures, to enable us to determine the latent heat of a given *volume* at any temperature. M. Regnault announces his intention of instituting researches for this object; but till the results are made known, we have no way of completing the data necessary for the present problem, except by estimating the density of saturated vapour at any temperature (the corresponding pressure being known by Regnault's researches already published) according to the approximate laws of compressibility and expansion (the laws of Mariotte and Gay-Lussac, or Boyle and Dalton). Within the limits of natural temperature in ordinary climates, the density of saturated vapour is actually found by Regnault (*Etudes Hygrométriques* in the *Annales de Chimie*) to verify very closely these laws; and we have reason to believe from experiments which have been made by Gay-Lussac and others, that as high as the temperature 100° there can be no considerable deviation; but our estimate of the density of saturated vapour, founded on these laws, may be very erroneous at such high temperatures as 230°. Hence a completely satisfactory calculation of the proposed scale cannot be made till after the additional experimental data shall have been obtained; but with the data which we actually possess, we may make an approximate comparison of the new scale with that of the air-thermometer, which at least between 0° and 100° will be tolerably satisfactory.

The labour of performing the necessary calculations for effecting a comparison of the proposed scale with that of the air-thermometer, between the limits 0° and 230° of the latter, has been kindly undertaken by Mr. William Steele, lately of Glasgow College, now of St. Peter's College, Cambridge. His results in tabulated forms were laid before the Society, with a diagram, in which the comparison between the two scales is represented graphically.

In the first table, the amounts of mechanical effect due to the descent of a unit of heat through the successive degrees of the air-thermometer are exhibited. The unit of heat adopted is the quantity necessary to elevate the temperature of a kilogramme of water from 0° to 1° of the air-thermometer; and the unit of mechanical effect is a metre-kilogramme; that is, a kilogramme raised a metre high.

In the second table, the temperatures according to the proposed scale, which correspond to the different degrees of the air-thermometer from 0° to 230°, are exhibited. [The arbitrary points which coincide on the two scales are 0° and 100°.]

Note.—If we add together the first hundred numbers given in the

first table, we find 135·7 for the amount of work due to a unit of heat descending from a body A at 100° to B at 0°. Now 79 such units of heat would, according to Dr. Black (his result being very slightly corrected by Regnault), melt a kilogramme of ice. Hence if the heat necessary to melt a pound of ice be now taken as unity, and if a *metre-pound* be taken as the unit of mechanical effect, the amount of work to be obtained by the descent of a unit of heat from 100° to 0° is $79 \times 135{\cdot}7$, or 10,700 nearly. This is the same as 35,100 foot-pounds, which is a little more than the work of a one-horse-power engine (33,000 foot-pounds) in a minute; and consequently, if we had a steam-engine working with perfect œconomy at one-horse-power, the boiler being at the temperature 100°, and the condenser kept at 0° by a constant supply of ice, rather less than a pound of ice would be melted in a minute.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

May 17, 1847.

A Theory of the Transmission of Light through Transparent Media, and of Double Refraction, on the Hypothesis of Undulations. By Professor Challis.

The object of the author in this, as in two preceding communications on *Luminous Rays* and on the *Polarization of Light* *, is, to establish the undulatory theory of light on hydrodynamical principles, by means of a system of ray-vibrations, the motions in which are mathematically deduced from hydrodynamical equations. In applying these views to the transmission of light through transparent media, it is assumed that the æther is of the same uniform density and elasticity within any transparent medium as without; and that the diminished rate of propagation in the medium is owing to the obstacle which its atoms oppose to the free motion of the æthereal particles. Considering the proximity of the atoms to each other, and that the retarding effect of each atom at a given instant extends through many multiples of its linear dimensions, it is presumed that the mean retardation, though resulting from the presence of discrete atoms, may be regarded as continuous. It is also supposed that the mean effect of the presence of the atoms is to produce an *apparent* diminution of the elasticity of the æther, the motion in all other respects being the same as in free space. By the application of these principles, it is first shown that the *surface of elasticity*, that is, the surface whose radius vector drawn in any given direction represents the elasticity in that direction, is in general an *ellipsoid*. This being ascertained, the velocity of a *ray* in any given direction is investigated; and the result is, that the surface whose radius vectors drawn in any given direction represent the velocities of propagation of two oppositely polarized rays in that direction, is precisely the wave-surface in Fresnel's theory of double refraction.

March 6, 1848.

A Mathematical Theory of Luminous Vibrations. By Professor Challis.

This paper is intended to be supplementary to three former com-

* See Lond. Ed. and Dubl. Phil. Mag. vol. xxx. p. 365.

munications in which the undulatory theory is treated on hydrodynamical principles, and to elucidate or confirm results previously arrived at. In particular the author enters more at length into the mathematical theory of ray-vibrations, which, according to his views, correspond to rays of light. The principal theoretical deductions are,—(1.) that the longitudinal vibrations of a ray are defined by a function of the form $\sin \frac{2\pi}{\lambda}\left(z - a\,t\sqrt{1+\frac{e\,\lambda^2}{\pi^2}}\right)$, λ being the breadth of the undulation, and a, e certain constants; (2.) that light from any source is in general *composed* of rays for which a and $\frac{e\lambda^2}{\pi^2}$ are the same and λ different; (3.) that light coming immediately from its origin is *common* light, whatever be the nature of the cause producing it, and that to become *polarized* light, it must be acted upon by reflexion, refraction, &c.; (4.) that light coming immediately from its origin is seen in all directions.

November 13, 1848.

Second Memoir on the Fundamental Antithesis of Philosophy. By W. Whewell, D.D.

This memoir is a continuation of a former one in which the antithesis of thoughts and things, of ideas and facts, of subjective and objective, were shown to be at bottom the same antithesis, and to be a fundamental antithesis, the union of the two elements entering into all knowledge, and their separation being the test of all philosophy. The present memoir is employed in illustrating the proposition that the progress of science consists in the transfer of some truth from the factorial to the ideal side of the antithesis, or as it may be termed, in the *idealization of facts.* This is exemplified in mechanics, astronomy, botany and chemistry.

In a note, the author remarks on certain German systems of philosophy with reference to this antithesis. The Sensatorial school having reduced all knowledge to facts, Kant re-established the necessity of Ideas, which Fichte made almost the exclusive element. Schelling founded his philosophy upon the *absolute*, from which he derives both facts and ideas, but which a wiser philosophy shows us that we can never reach; and Hegel took the same foundation, but in a certain degree rightly pointed out that the progress towards the identity of fact and idea is to be traced in the history of science; which view, however, he has carried into detail by rash and blind conjecture.

On the Elements of Plane Geometrical Trigonometry, applicable to Trigonometrical Formulæ. By the Rev. F. Calvert.

The object of this paper is to define as distinctly as possible the elementary terms of trigonometry, and to explain the conventional use of the negative sign in expressing such simple functions of angles as the sine, cosine, tangent, &c.

November 27, 1848.

On a Difficulty suggested by Professor Challis in the Theory of Sound. By Robert Moon.

In a paper by Professor Challis contained in the Supplementary Number of the 32nd Volume of the Philosophical Magazine, I find the following :—

"The difficulty respecting the augmentation of the velocity of sound by the development of heat, cannot be so summarily disposed of as Mr. Airy appears to imagine. I shall perhaps succeed better in conveying my meaning by using symbols. If θ be the temperature where the pressure is p and density ρ, and θ_1 the temperature in the quiescent state of the fluid, we have, by a known equation,

$$p = a^2\rho(1 + \alpha.\overline{\theta - \theta_1}).$$

Hence

$$\frac{d^2z}{dt^2} = -\frac{dp}{\rho dz} = -\frac{a^2 d\rho}{\rho dz} - a^2\alpha(\theta - \theta_1)\frac{d\rho}{\rho dz} - a^2\alpha\frac{d\theta}{dz}. \qquad (1.)$$

"The usual theory explains how the third term of the right-hand side of this equation may be in a given ratio to the first; but my difficulty is to conceive how the same can be the case also with the *second* term, since it changes sign with the change of sign of $\theta - \theta_1$."

I conceive that the explanation, according to the usual theory to which Professor Challis here alludes, depends upon the principle, "that for very small condensations of air, the rise of temperature will be proportional to the increase of density." (Vide Herschel On Sound, *Encyc. Met.*, art. 72.) Thus we may put

$$\theta - \theta_1 = k(1 - \rho),$$

where k is a constant, and 1 is put for the density of equilibrium: on which hypothesis it is obvious that the third term of equation (1.) will be a multiple of the first, as described by Prof. Challis. It also follows that the second term vanishes, since it has $(1 - \rho)$ for a factor, and in reducing (1.) to the ordinary form of the differential equation of sound the difference between ρ and 1 is neglected. It thus, I think, appears that the difficulty suggested by Prof. Challis has no real existence.

Observations of the Aurora Borealis of Nov. 17, 1848, made at the Cambridge Observatory. By Professor Challis.

These observations relate principally to the corona, or point of apparent convergence of the streamers, the remarkable display of Nov. 17 being peculiarly favourable for noting the position of this critical point. They were taken partly by estimation of distances from stars, and partly by a small altitude and azimuth instrument (called by the author a *meteoroscope*), which is furnished with a bar, eighteen inches long, carrying at one end a rectangular piece whose edges are horizontal and vertical, by looking at which through an eyelet-hole, about the size of the pupil of the eye, at the other end, the collimation is performed. Each observed position is compared

with the point of the heavens to which the south end of the dipping-needle was directed at the time of observation. This point was ascertained by means of observations of declination, horizontal force, and vertical force, taken at the Greenwich Observatory during the prevalence of the aurora by Mr. Brooke's photographic process, the results of which were communicated to the author by the Astronomer Royal. It is assumed that the magnetic declination and dip at Cambridge differ from those at Greenwich at any given time by certain constant quantities, whether the magnet be disturbed or not. These constant differences were derived from the following formulæ :—

$$V - V_0 = 0{\cdot}142518\lambda + 0{\cdot}159548l$$
$$D - D_0 = 0{\cdot}027713\lambda + 0{\cdot}513523l,$$

in which V and D are the declination and dip at a place not very distant from Greenwich, V_0 and D_0 the contemporaneous declination and dip at Greenwich, λ the longitude of the place *west*, in *seconds of time*, and l the *excess* of its latitude in *minutes* above that of Greenwich. These are merely formulæ of interpolation by simple differences derived from the following data :—

	Lat.	Long. West.	Declination in 1843.	Dip in 1843.
	° ′	m. s.	° ′	° ′
Greenwich	51 28·6	0 0·0	23 17·59	69 1·9
Makerstoun....	55 34·7	10 3·5	25 22·85	71 25·0
Dublin.....	53 21·0	25 4·0	27 9·87	70 41·3

The above are very accurate contemporaneous values of the declination and dip at the three places, and the formulæ derived from them will probably apply with considerable accuracy to any place in the United Kingdom at any date not very remote from 1843. For the Cambridge observatory $V - V_0 = +3'{\cdot}7$ and $D - D_0 = +22'{\cdot}0$.

The mean result from 24 observations of the position of the corona is, that it was situated 5′ further from the astronomical zenith, and 1° 14′ nearer to the meridian than the point of the heavens to which the south end of the dipping-needle was directed.

The places of the corona given by the different observations exhibit considerable discrepancies, which are accounted for by saying, that as the formation of the corona is merely an effect of *perspective*, its position varies, since the streamers are not exactly parallel, with the locality from which they rise ; also with any variation of their direction at a given locality ; and, supposing the course of the streamers to be somewhat curved in their ascent, it will vary with the height to which they rise. Accordingly, as appeared to be the fact, the corona would be continually shifting its position within certain limits.

Prof. Challis has made a similar comparison with observations of the position of the corona of the same aurora made at Haverhill, at Darlington, and at Bath ; also with observations at Whitehaven of the aurora of Oct. 18, 1848, and of that of Oct. 24, 1847, at Cam-

bridge. From a consideration of all the results derived from the discussion of observations made on different occasions and at different places, the following conclusions seem to be established:—

First, that the corona of an aurora borealis is formed near the point of the heavens to which the south end of the dipping-needle at the place of observation is directed.

Secondly, that the observations, while they indicate no decided difference of altitude between the two points, show with great probability that the corona is the more *westerly* by about $1\frac{1}{4}^\circ$ measured on an arc perpendicular to the meridian.

The paper concludes with a particular description of the aurora borealis of Nov. 17 as observed at the Cambridge Observatory, and with three tables of the observations of declination, horizontal force, and vertical force, made at Greenwich, and used in the calculations. These observations present so striking an instance of great magnetic disturbances occurring simultaneously with an extraordinary display of the aurora borealis, that the connexion in some way of the two kinds of phænomena must be regarded as a physical fact.

On Clock Escapements. By E. B. Denison, Esq., of Trinity College.

The object of this paper is, first to point out the real cause of the general excellence of the dead beat escapement; and secondly, to show that in a gravity, or remontoir escapement, in which the pendulum raises an arm carrying a small weight, from an angle γ up to its extreme semiarc α, which follows the pendulum down again to an angle β (either $+$ and less than γ, or $= -\gamma$), there is a certain proportion between α, β, and γ, which will cause the errors of the clock for small variations of α to be much smaller than in the dead escapement, and in fact inappreciable.

The author adopts the equations obtained by Mr. Airy in his paper on this subject in vol. iii. of the Transactions of the Society, and shows that the increase of the time of an oscillation

$$= \Delta\left(\frac{d\phi}{\phi} - \frac{3d\alpha}{\alpha}\right),$$

where Δ is the difference between the time of oscillation of a free pendulum and one affected by this escapement (which in clocks of the best construction he shows will amount to about 1 second a day); ϕ is the angular accelerating force of the escapement on the pendulum; $d\phi$ the variation in this force due to the variation of the friction of the train and of the state of the oil on the acting part of the pallets; $d\alpha$ the variation of the arc from the same causes, and also from the state of the oil on the dead or circular part of the pallets. It appears therefore that the two causes of error have a tendency to correct each other; and in practice it is found that $\frac{3d\alpha}{\alpha}$ is generally not far short of $\frac{d\phi}{\phi}$, which is the reason of these clocks going so well.

In a gravity escapement there is no variation of the force, and

the author shows from Mr. Airy's equations that $\frac{d\Delta}{d\alpha}=0$ if $\alpha=\gamma\sqrt{2}$ in that escapement where the remontoir weight is taken up at γ and follows the pendulum again to $-\gamma$; and in the other kind of gravity escapement $\frac{d\Delta}{d\phi}\frac{d\Delta}{d\alpha}=0$ when

$$\alpha^2=2\sqrt{\alpha^2-\gamma^2}\sqrt{\alpha^2-\beta^2}.$$

This last construction however is barely practicable, if this condition is to be satisfied, on account of the small difference between β and γ which is allowed by the deduction of the value necessary for $\alpha-\gamma$, the angle in which the unlocking of the escapement is effected; although this is the construction which has been used in nearly all the gravity escapements that have been tried; and of course the proper condition has been very far from satisfied, and the clocks have failed.

In a supplement to this paper the author proposes, chiefly for turret clocks, a new construction of a spring remontoir on the axis of the escape-wheel. The object of such remontoirs is to remove from the escapement (of any ordinary kind) the great inequalities of force caused by the varying friction of the heavy train and dial-work, and by the action of the wind on the hands; and also to cause the minute-hand to move only at visible intervals, such as $\frac{1}{2}$ a minute, and the striking to take place exactly at the right second. The Royal Exchange clock, made under the superintendence of the Astronomer Royal, has a gravity remontoir in the train introduced for these purposes; but it is too complicated and expensive for ordinary use, and has a good deal of friction, from which the proposed remontoir is free. Spring remontoirs winding up at similar intervals have been tried in France, but without success, from defects in their construction.

December 11, 1848.

On the Formation of the Central Spot of Newton's Rings beyond the Critical Angle. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

It has long been known that when Newton's rings are formed between the under surface of a prism and the upper surface of a lens, or of a second prism, so as to allow of increasing the angle of incidence at pleasure, the rings disappear when the critical angle is passed, but the central spot remains. The existence of the spot under these circumstances has even been attributed to the disturbance in the second medium, which, when the angle of incidence exceeds the critical angle, takes the place of that disturbance which at a smaller incidence constitutes the refracted light; but the expression for the intensity has not hitherto been given, so far as the author is aware. The object of the author in the present paper is to supply this deficiency.

The author has not adopted any particular dynamical theory, but has deduced his results from Fresnel's expressions for the intensities of reflected and refracted polarized light. When the angle of incidence becomes greater than the critical angle these expressions become imaginary. When the imaginary expressions are interpreted in the way in which physical considerations show that they must be interpreted, it becomes easy to obtain the expression for the intensity of the light, whether reflected or transmitted, in the neighbourhood of the spot. When the first and third media are of the same nature, the following expression is obtained for the intensity (I) of the reflected light, the incident light being polarized in the plane of incidence, and its intensity being taken for unity,

$$I = \frac{(1-q^2)^2}{(1-q^2)^2+4q^2\sin^2 2\theta}, \text{ where } q = \varepsilon^{-\frac{2\pi D}{\lambda}\sqrt{\mu^2\sin^2 i-1}}.$$

In this expression μ is the refractive index of the first medium, i the angle of incidence on the surface of the second medium, or interposed plate of air, D the thickness of that plate at the point considered, λ the length of a wave in air, 2θ the acceleration of phase due to total internal reflexion. When the light is polarized perpendicularly to the plane of incidence, it is only necessary to replace 2θ by 2φ, the angles θ, φ being those so denoted in Airy's Tract. The intensity of the transmitted light is obtained by subtracting that of the reflected light from unity.

From the expression for the intensity, the author has deduced the following results, all of which he has verified by observation.

The spot is comparatively large near the critical angle, and becomes smaller and smaller as the angle of incidence increases. Near the critical angle the fainter portion, or *ragged edge*, of the bright spot seen by transmission is broad; at considerable angles of incidence the light decreases with comparative abruptness. Towards the edge of the spot there is a predominance of the colours at the red end of the spectrum, causing the ragged edge to appear brown. Near the critical angle the spot is larger for light polarized perpendicularly to the plane of incidence than for light polarized in that plane: at considerable angles of incidence the order of magnitude is reversed. The difference is far more conspicuous in the former case than in the latter, and in that case consists principally in the greater extent of the ragged edge. When the incident light is polarized at an azimuth of 45°, or thereabouts, and the transmitted light is analysed so as to extinguish the light transmitted near the point of contact, there is seen a central dark patch surrounded by a luminous ring.

February 26, 1849.

On a New Method of finding the Rational Roots of Numerical Equations. By Robert Moon, Esq.

The author proposes to found a new experimental method of find-

ing the integral roots of numerical equations upon the following theorem.

If the equation

$$x^n+p_1x^{n-1}+p_2x^{n-2}+\text{ \&c. }+p_ix^{n-i}+\text{ \&c.}+p_n=0$$

has a positive and integral root m, we shall have $-p_n$ equal to m terms of the following series :—

$$\begin{aligned}
&A_{n-1}+1.\,A_{n-2}+1.2.\,A_{n-3}+1.2.3\,A_{n-4}+\text{ \&c. }+1.2\ldots(n-i+1)\,A_i\\
&\quad+\text{ \&c. }+1.2\ldots(n-1)\,A_0\\
&+A_{n-1}+2.\,A_{n-2}+2.3\,A_{n-3}+2.3.4\,A_{n-4}+\text{ \&c. }+2.3\ldots(n-i+2)\,A_i\\
&\quad+\text{ \&c. }+2.3\ldots n\,A_0\\
&+A_{n-1}+3.A_{n-2}+3.4\,A_{n-3}+3.4.5\,A_{n-4}+\text{ \&c. }+3.4\ldots(n-i+3)\,A_i\\
&\quad+\text{ \&c. }+3.4\ldots(n+1)\,A_0\\
&+A_{n-1}+4.\,A_{n-2}+4.5\,A_{n-3}+4.5.6\,A_{n-4}+\text{ \&c. }+4.5\ldots(n-i+4)\,A_i\\
&\quad+\text{ \&c. }+4.5\ldots(n+2)\,A_0\\
&\qquad\text{\&c.}\qquad\qquad\qquad\text{\&c.}
\end{aligned}$$

where

$$A_i=(n-i)(p_i-h_1p_{i-1}+h_2p_{i-2}+\text{\&c.}+(-1)^rh_rp_{i-r}+\text{\&c.}\pm h_{i-1}p_1\mp h_i)$$

and

h_1=the sum of the natural numbers $1, 2, 3\ldots\ldots(n-i)$.

h_2=the sum of the homogeneous products of the same quantities of two dimensions.

h_3=the sum of the homogeneous products of the same quantities of three dimensions, and so forth.

From the above formula for A_i may be determined all the coefficients A except the first, which is determined from the equation

$$A_{n-1}=p_{n-1}-p_{n-2}+p_{n-3}-\text{\&c.}\pm p_1\mp 1.$$

Having determined the quantities A in any particular case, let them be substituted in the first line of the series. If the sum of that line be equal to $-p_n$ unity is a root of the equation. Let the second line be then written down and added to the first. If the sum of the two equals $-p_n$, 2 is a root of the equation, and so by adding successive lines we shall ascertain whether the successive integers 3.4., &c. are or are not roots of the equation.

The quantities h in the expression for A_i depend upon the number of the coefficient and the number of the dimensions of the equation. The author proposes that these should be calculated and tabulated for equations of all dimensions up to a certain limit, by which means we should be in possession of so many skeletons of equations, ready for application in any particular case, and the calculation in particular instances would be thus greatly facilitated.

It will be observed that each successive line is derived from that preceding by a simple division and multiplication of the separate terms of the latter, and thus each succeeding trial facilitates those which follow ; contrary to what obtaind in the ordinary method by successive substitutions, in which each attempt proceeds *de novo*.

If the addition of a term makes the series from being greater than p_n less than it, or *vice versa*, a fractional or surd root will lie between

the number expressing the number of the term so added and the number next below it.

If all the roots are impossible, the series will be either always greater or always less than p_{∞}, whatever be the number of terms taken.

For an example take the cubic

$$x^3+px^2+qx+r=0.$$

Here
$$\begin{aligned}-r=&3\times 1.2+2(p-3)1+q-p+1\\&+3\times 2.3+2(p-3)2+q-p+1\\&+3\times 3.4+2(p-3)3+q-p+1\\&+\text{ \&c. to } x \text{ terms,}\end{aligned}$$

if x is a positive integer.

The method in common with other experimental methods applies to the discovery of all roots, possible or impossible, which do not involve surds.

March 12, 1849.

On the Intrinsic Equation to a Curve, and its application. By the Master of Trinity.

The author remarked that the expressions for the lengths of curves, their involutes and evolutes, in the ordinary methods, are complex and untractable, which arises in a great measure from the properties of *extrinsic* lines being introduced, namely, coordinates. But a curve may be represented without any such additions, by an equation between the length and the angle of flexure, which is therefore called the *intrinsic* equation. This equation gives, with remarkable facility, the radii of curvature; involutes and evolutes of most curves. It also expresses very simply what may be called *running* curves; namely, curves which run like a pattern along a strip of ornamented work. A very simple equation expresses, for instance, the inclined scroll pattern so common in the antique, and by altering the constants, gives to this pattern an endless variety of forms. If s be the length of the curve and ϕ the angle, the *intrinsic* equation to the circle is $s=a\phi$; to the cycloid $s=a\sin\phi$. The equation to an epicycloid or hypocycloid is $s=a\sin m\phi$, according as m is less or greater than unity. The equation to an undulating pattern is $\phi=m\sin s$, which assumes very various shapes by varying m. The method was also used in proving that if we take the successive involutes of a curve an indefinite number of times, the resulting curve (with certain limitations) tends to become *the equiangular spiral* if the unwrapping be always in the same direction, and tends to become *the cycloid* if the unwrapping be alternately in opposite directions. The latter proposition had already been discovered by Bernouilli and proved by Euler.

April 23, 1849.

On the Variation of Gravity at the Surface of the Earth. By G. G. Stokes, M.A., Fellow of Pembroke College, Cambridge.

In the theory of the figure of the earth on the hypothesis of original fluidity, a simple expression is obtained for the variation of gravity along the surface, which contains the numerical relation between the ellipticity and the ratio of polar to equatoreal gravity, known as Clairaut's theorem. The demonstration, however, of this expression does not require the hypothesis of original fluidity, if the spheroidal form of the surface and its perpendicularity to the direction of gravity be assumed as results of observation. On the hypothesis merely that the earth consists of nearly spherical strata of equal density, Laplace has established a connexion between the form of the surface, regarded as a surface of equilibrium, and the variation of gravity along it; and in the particular case in which the surface is an oblate spheroid of small ellipticity, having its axis of figure coincident with the axis of rotation, the expression which results for the variation of gravity is identical with that which is obtained on the hypothesis of original fluidity. The object of the author in the first part of this paper is to obtain the general connexion between the form of the surface and the variation of gravity along it, by an application of the doctrine of potentials, without making any hypothesis whatsoever respecting the distribution of matter in the interior of the earth.

The latter part of the paper was devoted to the consideration of the irregularities produced in the variation of gravity by the irregular distribution of land and sea at the surface of the earth. The author has shown why gravity should appear less on continents than on small islands situated at a distance from any continent, which is a circumstance that has long since been observed. The result is accounted for by the elevation of the sea-level produced by the attraction of a continent, in consequence of which a station on a continent is further removed from the centre of the earth than it appears to be. It is shown also that the numerical value of the earth's ellipticity, which has been deduced from pendulum experiments, is somewhat too great, in consequence of the undue proportion of oceanic stations in low latitudes, among the group of stations at which the observations were made which have been employed in the discussion.

The author has given formulæ whereby observed gravity may be corrected for the irregularities of the earth's surface. These formulæ require a knowledge, or at least an approximate knowledge, of the height of the land and the depth of the sea throughout the earth's surface. The sign and magnitude of the difference between observed gravity, and gravity calculated on the hypothesis of the earth's original fluidity, appears on the whole to depend on the insular or continental character of the station at which the observation has been taken. This circumstance renders it probable, that if observed gravity were corrected for the irregular attraction due to the irregular

distribution of sea and land throughout the whole surface of the earth, the result would agree far better with gravity calculated on the hypothesis of original fluidity.

May 7, 1849.

Additional Note to a Memoir on the Intrinsic Equation of Curves. By Dr. Whewell.

This note contained an extension of a theorem discovered by John Bernouilli, and demonstrated by Euler, to this effect: that if from any *rectangular* curve a string be unwrapped, and from the curve so described again a string unwrapped, and so on perpetually and alternately in opposite directions, the curves constantly tend to the form of the common cycloid. The extension is to this effect: that if the original curve be not rectangular, the curves perpetually tend to the form of an epicycloid or hypocycloid, according as the angle is greater or less than a right angle.

May 21, 1849.

Discussion of a Differential Equation relating to the breaking of Railway Bridges. By G. G. Stokes, M.A., Fellow of Pembroke College.

In August 1847 a Royal Commission was appointed "for the purpose of inquiring into the conditions to be observed by engineers in the application of iron in structures exposed to violent concussions and vibration." Among other branches of inquiry, the members of the Commission have lately been making experiments on the motion of a carriage, variously loaded in different experiments, which passed with different velocities over a slight iron bridge; the object of the experiments being to examine the effect of the velocity of a train in increasing or decreasing the tendency of a bridge over which the train is passing to break under its weight. The remarkable result was obtained, that the deflection is in some cases much greater than the central statical deflection, and that the greatest deflection takes place after the body has passed the centre of the bridge. In investigating the theory of the motion, reducing the problem to the utmost degree of simplicity by regarding the moving carriage as a heavy particle, and neglecting the inertia of the bridge, Professor Willis, who is a member of the Commission, was led to a differential equation of the form

$$\frac{d^2y}{dx^2} = a - \frac{by}{(2cx - x^2)^2},$$

where x, y are the horizontal and vertical co-ordinates of the moving body, $2c$ is the length of the bridge, and a, b are certain constants. Professor Willis requested the author's consideration of this equation, with a view to obtain numerical results, and to determine, if possible, the velocity which produces a maximum deflection.

The author has expressed y in a series according to ascending

powers of x, which is convergent when $x < 2c$. The convergency, however, becomes very slow when x approaches the limit $2c$; and the series does not point out the law according to which $f(x)$ or y approaches its extreme value 0 as x approaches $2c$. When the constant term in the second member of the preceding equation is omitted, the equation may be integrated in finite terms; and consequently the variables can be separated in the actual equation, so that $f(x)$ can be expressed explicitly by means of definite integrals. In this way the author has obtained $f(2c-x)-f(x)$ in finite terms, so that the numerical value of $f(x)$ may readily be obtained from $x=c$ to $x=2c$, after it has been calculated from the series from $x=0$ to $x=c$: and between these limits the series is very convergent, being ultimately a geometric series with a ratio $\frac{x}{2c}$. The author has also investigated a series proceeding according to ascending powers of $c-x$, which converges more rapidly than the former when x approaches c. By the use of these two series, $f(x)$ may be calculated by means of series which are ultimately geometric series, with ratios ranging from 0 to $\frac{1}{4}$.

The unsymmetrical form of the trajectory, and the largeness of the deflection produced by the moving body, come out from the investigation. By means of the numerical values of $f(x)$ the author has drawn a figure representing the trajectory for four different velocities. The expression for the central deflection, however, becomes infinite when x becomes equal to $2c$, which shows that it is necessary to take into account the inertia of the bridge; although, if the bridge be really light, the solution obtained when the inertia of the bridge is neglected may be sufficiently exact for the greater part of the body's course.

On Hegel's Criticism of Newton's *Principia*. By Dr. Whewell.

Parts of Hegel's *Encyclopædia* are here examined with the purpose of testing the value of his philosophy, not of defending Newton. Hegel says that the glory due to Kepler has been unjustly transferred to Newton; confounding thus the discovery of the laws with the discovery of the force from which the laws proceed, in which latter discovery Kepler had no share. Hegel pretends to derive the Newtonian "formula" from the Keplerian law, thus;—by Kepler's law, A being the distance, and T the periodic time, $\frac{A^3}{T^2}$ is constant: but Newton (Hegel says) *calls* $\frac{A}{T^2}$ universal gravitation, whence universal gravitation is inversely as A^2:—a most absurd misrepresentation of the course of Newton's reasoning. In the same manner Hegel criticises, and utterly misrepresents Newton's explanation, for the elliptical orbit, of the body's approaching to and receding from the centre; and of the reason why the body moves in an ellipse. Hegel also offers his own explanation of Kepler's laws from his own *à priori* assumptions. He says that the motion of the heavenly bodies is not a being pulled this way or that, as is imagined by the Newtonians; they go along, as the ancients said, like blessed gods.

Part I. Vol. IX. of the SOCIETY'S TRANSACTIONS, containing the Memoirs of which Abstracts appear in this Number of the Proceedings, is just published. It may be had at the Society's Rooms from the Curator, and is sold by Messrs. Deighton, Cambridge, and Mr. J. W. Parker, Strand, London. The price to the Fellows of the Society is 10*s.*, *and to the Public* 12*s.*

Any Part of any preceding Volume may be had separately at the above-mentioned places.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 26, 1849.

On the Dynamical Theory of Diffraction. By Professor Stokes.

The problem of diffraction is treated mathematically by conceiving each wave of a series incident on a small aperture, or passing the edge of a diffracting body, broken up on arriving at the aperture or diffracting edge, regarding each element of the wave as the centre of an elementary disturbance, which diverges spherically from that element, and finding by integration the aggregate disturbance at any point in front of the primary wave. With the exception of one case of diffraction, which will be mentioned further on, the illumination in front of an aperture is insensible except in the immediate neighbourhood of a normal to the primary wave drawn through a point in the aperture. Consequently we are only concerned with the law of disturbance in that part of a secondary wave which lies very near the normal to the primary wave; the nature of the disturbance in other directions does not affect the result, since the secondary waves neutralize each other by interference. Now it has been shown by others, by indirect methods, that if c be the coefficient of vibration in the incident light, dS an element of the area of the aperture, r the radius of a secondary wave diverging from dS, λ the wave length, the coefficient of vibration in the secondary wave will be $\frac{cdS}{\lambda r}$, and the phase of vibration must be accelerated by a quarter of an undulation; or in other words, $\frac{\lambda}{4}$ must be subtracted from the retardation due to the radius r. These results, however, according to what has been already remarked, only apply to that portion of a secondary wave which lies immediately about the normal to the primary. The object of the author in this paper was to determine, on purely dynamical principles, the law of disturbance *in any direction* in a secondary wave.

The author has treated the æther as an elastic solid; and as such it must be treated in considering light, if the theory of transverse vibrations be not rejected. The object which he had in view required the solution, in the first instance, of the following problem;—to determine the disturbance at any point of an elastic medium, and

at any time, due to a given small arbitrary disturbance confined to a finite portion of the medium. This problem was solved long ago by Poisson; but the author has given a totally different solution of it, which appears to be in some respects simpler than Poisson's. In the course of the solution, the author was led incidentally to the following very general dynamical theorem.

Let any material system whatsoever, in which the forces acting depend only on the positions of the particles, be slightly disturbed from a position of equilibrium, and then left to itself: then the part of the disturbance at any time which depends on the initial displacements will be got from that which depends on the initial velocities by differentiating with respect to the time, and replacing the arbitrary functions, or arbitrary constants, which express the initial velocities by those which express the corresponding initial displacements. Particular cases of this theorem are of frequent occurrence, but the author is not aware of any writing in which the theorem is enunciated in all its generality.

The problem above-mentioned has been applied by the author to the case of diffraction in the following manner. Conceive a series of plane waves of plane-polarized light propagated in a direction perpendicular to a fixed mathematical plane P. According to the principle of the superposition of small motions, we have a perfect right to consider the disturbance of the æther in front of the plane P as the resultant of the elementary disturbances corresponding to the several elements of P. Let it be required to determine the disturbance which corresponds to an elementary portion only of the plane P. In this consists the whole of the dynamical part of the theory of diffraction, if we except the case of diffraction at the common surface of two different media; the rest is a mere question of integration. Let the time t be divided into equal intervals, each equal to τ. The disturbance which is propagated across the plane P during the first interval τ occupies a layer of the medium having a thickness $v\tau$, if v be the velocity of propagation, and consists of a certain velocity and a certain displacement. By the problem above mentioned, we can find by itself the effect of the disturbance which occupies so much only of this layer as corresponds to a given element dS of P. By doing the same for the 2nd, 3rd, &c. intervals τ, and then making the number of such intervals increase and their magnitude decrease indefinitely, we shall get the effect of the disturbance which is continually transmitted across dS. The result is a little complicated, but is much simplified when certain terms are neglected which are only sensible when the radius of the secondary wave is comparable with λ, and which are wholly insensible in the physical applications of the problem. The result thus simplified may be enunciated as below:—In the enunciation, the term *diffracted ray* is used to denote the disturbance in an elementary portion of a secondary wave, diverging in a given direction from the centre; the plane containing the incident and diffracted rays will be called the *plane of diffraction*, the supplement of the angle between these two rays the *angle of diffraction*, and the plane passing

through a ray of plane-polarized light and containing the direction of vibration the *plane of vibration*.

The incident ray being plane-polarized, each diffracted ray will be plane-polarized, and the plane of polarization will be determined by the following law:—*The plane of vibration of the diffracted ray is parallel to the direction of vibration of the incident ray.* The direction of vibration being thus determined, it remains only to specify its magnitude. Let

$$\zeta = c \sin \frac{2\pi}{\lambda} (vt - x)$$

be the displacement in the case of the incident light, ζ' the displacement in the case of the diffracted ray, ζ' being reckoned positive in the direction which makes an acute angle with that in which ζ is reckoned positive. Let r be the radius of the secondary wave diverging from $d\mathrm{S}$, and let r make angles θ with the direction of propagation of the incident ray, and ϕ with the direction of vibration; then

$$\zeta' = \frac{cd\mathrm{S}}{2\lambda r} (1 + \cos\theta) \sin\phi \cos \frac{2\pi}{\lambda} (vt - r) \quad . \quad . \quad . \quad \text{(a.)}$$

When an arbitrary function of $vt - x$, $f(vt - x)$ occurs in ζ, it is not $f(vt - r)$ but $f'(vt - r)$ that appears in ζ', where f' denotes the derivative of f, and accordingly in the particular case in which $f(u) = \sin u$ the sine in ζ is replaced in ζ' by a cosine. It may readily be verified, that if the formula (a.) be applied to determine by integration the disturbance which corresponds to the whole of the plane P, the disturbance in front is the same as if the wave had not been supposed broken up, and no disturbance is propagated backwards.

The law obtained for determining the position of the plane of polarization of the diffracted ray seems to lead to a crucial experiment for deciding between the two rival theories between the directions of vibration in plane-polarized light. Suppose the incident light polarized by transmission through a Nicol's prism mounted in a graduated instrument, and let the diffracted light be analysed in a similar manner. By means of the graduation of the polarizer, we can turn the plane of polarization of the incident ray, and consequently the plane of vibration, which is either parallel or perpendicular to the plane of polarization, round through equal angles of say 5° or 10° at a time. According to theory, the *planes of vibration* of the diffracted ray will not be distributed uniformly, but will be crowded towards the perpendicular to the plane of diffraction. But experiment will enable us to decide whether the *planes of polarization* are crowded towards the plane of diffraction or towards the perpendicular to the plane of diffraction, and we shall accordingly be led to conclude, either that the vibrations are perpendicular, or that they are parallel to the plane of polarization.

In ordinary cases of diffraction, the illumination, in consequence of interference, is insensible beyond a small angle of diffraction. It is only by means of a fine grating that we can procure light of considerable intensity that has been diffracted at a large angle. The

author has been enabled to perform the experiment, or rather a modification of it, by the kindness of his friends Professors Miller and O'Brien; of whom the former lent him a fine glass-grating, consisting of a glass plate on which parallel and equidistant lines had been ruled with a diamond at the rate of 1300 to the inch, and the latter lent him the graduated instruments required. The theory does not quite meet the case of a glass-grating, in which the diffraction takes place at the common surface of two media, but it leads to a definite result on each of the two extreme suppositions:—1st, that the diffraction takes place before the light reaches the grooves; 2nd, that it takes place after the light has passed them; and the results are very different according as one or other of the two rival theories is adopted. In the principal experiments, the plane of the plate was placed perpendicular to the incident light, and the light observed was that which had been diffracted by transmission through the plate. The angle of diffraction, by which is meant the angle measured in air, ranged in the different experiments from about 20° to 60°. The result obtained was, that when the grooved face was turned towards the eye, there was a very sensible crowding of the planes of polarization of the diffracted light towards the plane of diffraction. When the grooved face was turned towards the incident light, there was a considerable crowding in the same direction, much more than in the other case. Since the effect of refraction, considered apart from diffraction, would be to crowd the planes in the contrary direction, the result seemed decisive in favour of Fresnel's hypothesis, that the vibrations are perpendicular to the plane of polarization. On the other hypothesis, diffraction would have conspired with refraction to produce a large crowding in a direction contrary to that in which the observed crowding took place. The amount of crowding, in both positions of the plate, was nearly what would be given by theory, on adopting Fresnel's hypothesis, and supposing that the diffraction took place *before* the light reached the grooves, but appeared in both cases a little less. The difference, however, was comprised within the limits of uncertainty depending upon the errors of observation and the error in the assumed value of the refractive index of the glass plate.

December 10, 1849.

Impact on Elastic Beams. By Homersham Cox, Esq., B.A., Jesus College.

Among the experiments instituted by the Royal Commission appointed to inquire respecting the use of iron in railway structure, was a series relating to impact on beams. These experiments were undertaken by Professor Hodgkinson, and were conducted in the following manner. The two ends of the beam were fixed in a horizontal position, and the blow was given against one of its vertical sides in a horizontal direction. The instrument for giving the blow

was a heavy iron ball, hanging down, when at rest, from a point of suspension vertically above the centre of the beam. The ball was raised through different arcs, and after descending by its own gravity, struck the beam. The deflection corresponding to different arcs of descent were carefully noted by a graduated scale.

The object of the present paper is to show that the results might have been predicted by known theoretical principles with considerable precision and confidence. The problem is divided into two parts:—1st, to estimate the amount of velocity lost by the ball at the first instant of collision; 2nd, to ascertain the effect of the elastic forces of the beam in destroying the *vis viva* which the whole system has immediately after collision. In the first part of the investigation, a general formula, derived from the combination of D'Alembert's principle and that of virtual velocities, is given for the motion of any material system subject to impact. The requisite geometrical condition required for the application of this general formula to the present case is obtained by the assumption, that immediately after impact the form of the beam is a gradual and tolerably uniform curve, such as, for example, the elastic curve of equilibrium. In this way it is determined that about one-half the inertia of the beam is effectively applied at the instant of collision to retard the ball.

The *vis viva* of the whole system thus computed is destroyed by the elastic forces of the beam developed by deflection. These, in the second part of the problem, are assumed to vary as the amount of central deflection. By the principle of *vis viva* a formula is easily obtained, connecting the amount of total deflection with the *vis viva* of the system immediately after collision.

Tables are given in which the theoretical and experimental results are compared. The correspondence is of the closest and most satisfactory nature. Indeed the theoretical result generally differs less from the mean of several experiments than those experiments differ among themselves. Both in the theoretical and experimental inquiries, every possible variation of the elements of the investigation —the relative masses of the beam and ball—the velocity of the latter —the rigidity and dimensions of the former—have been included.

February 11, 1850.

A paper was read by the Master of Trinity, "Criticism of Aristotle's account of Induction."

The passage criticised was *Analyt. Prior.* 11. 25, and is by Aristotle illustrated by this example. Elephant, horse, mule, &c., are long-lived; but elephant, horse, mule, &c. have no gall-bladder. If we suppose that the latter proposition may be converted and put in this form, "all animals which have no gall-bladder are as elephant, horse, mule, &c.," we may draw the conclusion that all animals which have no gall-bladder are long-lived. This convertibility and generalization of the second proposition are the necessary conditions for translating

induction into syllogism. And Aristotle really contemplated such a generalizing induction. He did not contemplate what has been called *inductio per enumerationem simplicem*, which is really no induction at all. This was shown to be so by reference to the case, often used as an example of induction, of the inference of Kepler's laws from the observation of the separate planets. It may be objected that the reasoning in such cases is inconclusive; and to this it is replied, that induction, as *reasoning, is* inconclusive. It is a source of truth different from reasoning; of first truths, the bases of reasonings, as Aristotle has remarked.

February 25, 1850.

On the Symbols of Logic, the theory of the Syllogism, and in particular of the Copula, and the application of the Theory of Probabilities to some questions of evidence. By Professor De Morgan.

This paper, which is in continuation of the one published in vol. viii. part 3 (read Nov. 9, 1846), and of subsequent additions contained in the author's work on *Formal Logic*, is divided into six sections.

Section I. *On the approximation of logical and algebraical modes of thought.*—The subjects of this section are,—1st, some development of the idea that the oppositions of logic have affinities which may one day lead to a connected theory, making use of a common instrument, just as the oppositions of quantity which are considered in algebra are connected by the general theory of the signs + and −; and 2nd, some remarks on the resemblance of the instrumental part of inference to algebraic elimination.

Ten such instances as affirmative and negative, conclusive and inconclusive, &c., are compared with the logical distinction of *universal* and *particular*; and it is pointed out, in all the cases in which it is not already acknowledged, that it would be *possible* to use any one of the ten in place of the last.

Section II. *On the formation of symbolic notation for propositions and syllogisms.*—Exclusive of remarks on the Aristotelian notation and on notation in general, and a statement for comparison of Sir William Hamilton's notation, this section contains the following matters.

1. A pictorial or diagrammatic representation of syllogistic inferences, being after the method pursued by Lambert, with such additions as will enable the system to represent all the cases in which contraries are used.

2. An abbreviated and arbitrary method of representing propositions and syllogisms.

Following Sir William Hamilton in making the quantity of both subject and predicate matter of symbolic expression, Mr. De Morgan gives his system of notation two new features. First, he dispenses with the representatives of the terms (except when it may be con-

venient to introduce them for the time), and represents the proposition by the *symbols of quantity only*, and the presence or absence of a sign of negation. Secondly, instead of making the symbols of universal and particular absolute, he gives one symbol,), to a universal subject and a particular predicate, and another, (, to a particular subject and a universal predicate: a dot [.] signifying negation. Thus X)·(Y, or simply)·(, represents 'No X is Y': X(·(Y, or (·(, represents 'Some Xs are not any Ys:' X()Y represents 'Some Xs are Ys.' Of the second circumstance above mentioned, Mr. De Morgan believes that it makes the rules easier, and knows that it makes the notation more suggestive.

Retaining in mind the order XY, YZ, XZ, which is the only *figure* used in the classification (being the first with inverted order of premises), the syllogism is to be denoted by the junction of the propositional symbols. Thus))))=)) denotes 'Every X is Y, every Y is Z, therefore every X is Z.' When this is to be read in any figure, the subject-quantities are to have their symbols thickened, the second premise being read first: thus in the four figures, in order, will be seen such symbols as ||||, ||||, ||||, ||||.

Section III. *On the symbolic forms of the extension of the Aristotelian system in which contraries are introduced.*—This system is the one which was completed and published to the Society *before* any correspondence with Sir William Hamilton. Mr. De Morgan remarks that it contains (incidentally, not designedly) every distribution of quantifications; and gives his reasons for not dwelling on this fact while the controversy was unfinished, with his statement that it had not struck him when the controversy began. Mr. De Morgan frequently distinguishes *this* system from Sir W. Hamilton's by calling the former that of *introduction of contraries*, the latter that of *invention of predicates*. For distinctness, it may be stated that Mr. De Morgan's other, or *numerically definite* system (the one concerned in the discussion), does not appear in the present paper, except as matter of allusion.

The forms of predication in this system are as follows, with reference to the order XY, x and y being not X and not Y.

Universals.

Affirmative	A_1	X))Y		Every X is Y
	A^1	$x))y$	or X((Y	Every Y is X
Negative	E_1	X))y	or X)·(Y	No X is Y
	E^1	$x))$Y	or X(·)Y	Everything is X or Y or both.

Particulars.

Affirmative	I_1	X()Y		Some Xs are Ys
	I^1	$x()y$	or X)(Y	Some things are neither Xs nor Ys
Negative	O_1	X()y	or X(·(Y	Some Xs are not Ys
	O^1	$x()$Y	or X)·)Y	Some Ys are not Xs.

Various rules of connexion are given, being all translations of those in the work on *Formal Logic*, except a classification of the particulars by *probability*, answering to that of universals. Thus of X))Y and X(·)Y, each makes the other *impossible*: of their con-

traries X(·(Y and X)(Y, each, so far as it affects the other, reduces its probability.

It appears that a quantified term has a quantified contrary: that of 'Every X' is 'some *x*s,' &c.

The symbolic *canon of validity* is;—if both middle parentheses turn the same way, there need be one universal proposition; if different ways, two. Thus)))(and (·))·(both have inferences; and so has)·()); but)·()(has none. The symbolic *canon of inference* is;—erase all signs of the middle term, and what is left (two negations, if there, counting as an affirmation) shows the inference. Thus from X(·)Y)·(Z we infer X(··(Z or X((Z: more simply, from (·))·(we infer ((.

Section IV. *On the symbolic forms of the system in which all the combinations of quantity are introduced by arbitrary invention of forms of predication* (Sir W. Hamilton's).

The modes of predication *peculiar* to this system have the same symbols,)(and (·), as the peculiar propositions of the system of contraries; but with very different significations, as follows:—

Contraries.	*Invention of predicates.*
(·) Universal negative with particular terms, and affirmative form in common language. *All things are either Xs or Ys.*	(·) Particular negative with particular terms, not used in common language. *Some Xs are not some Ys.*
)(Particular affirmative with universal terms, and negative form in common language. *Some things are neither Xs nor Ys.*	)(Universal affirmative with universal terms, being declaration of identity in common language. *All Xs are all Ys.*

Mr. De Morgan argues that Sir William Hamilton's system cannot be called an *extension* of that of Aristotle, in the sense in which that word is used.

The forms of predication are as follows:—

$A_1 + A^1$	)(All Xs are all Ys	E_1	)·(No Xs are Ys
I_1	() Some Xs are some Ys	—	(·) Some Xs are not some Ys
A_1	)) All Xs are some Ys	O^1	)·) No Xs are some Ys
A^1	((Some Xs are all Ys	O_1	(·(Some Xs are no Ys.

Previously to entering upon the forms of syllogism, Mr. De Morgan repeats and reinforces the objections brought forward in his *Formal Logic*; namely, that)(is a compound of)) and ((, and has no simple contradiction in the system; and that (·) not only has no simple contradiction, but cannot be contradicted except when the terms are singular and identical. He then proceeds to propose one mode of remedying these defects. Calling the ordinary proposition *cumular*, he proposes to make it *exemplar*, as asserting or denying of one instance only. In the universal proposition, the example is *wholly indefinite, any one*; in the particular proposition it is *not wholly indefinite, some one*. The defects of contradiction are thus entirely removed, as in the following list, in which each universal proposition is followed by its contradiction.

)(Any one X is any* one Y	((Some one X is any one Y
(·) Some one X is not some one Y	)·) Any one X is not some one Y
)) Any one X is some one Y	)·(Any one X is not any one Y
(·(Some one X is not any one Y	() Some one X is some one Y

In both systems there are thirty-six valid syllogisms, and in both the canon of validity is,—one universal (or wholly indefinite) middle term, and one affirmative proposition. But the symbolic canons of inference differ as follows (with reference to the order XY, YZ, XZ).

Exemplar system.—Erase the middle parentheses, and the symbol of the conclusion is left: thus))·) gives (·).

Cumular system.—Erase the middle parentheses, and then if both the erased parentheses turn the same way, turn any universal parenthesis which turns the other way, unless it be protected by a mark of negation. Thus)·(() gives)·), ())(gives (), and))·(gives (·(.

Section V. *On the theory of the copula, and its connexion with the doctrine of figure.*—In his work on *Formal Logic*, Mr. De Morgan had analysed the copula, and abstracted what he calls the *copular conditions* of the relation connecting subject and predicate. These are, *transitiveness*, seen in such copulæ as *support, govern, is greater than*, &c., *ex. gr.* if A govern B, and B govern C, A governs C: and *convertibility*, seen in such copulæ as *is acquainted with, agrees with*, &c.; *ex. gr.* if A agree with B, B agrees with A. Mr. De Morgan's position is, that any mode of relation which satisfies both these conditions has as much claim to be the copula as the usual one, *is*, which derives its fitness entirely from satisfying the above conditions. So far the work cited. In the present paper the *correlative* copula is introduced, as *is supported* in opposition to *supports*, &c., and every system of syllogism is thus extended. If a copula be taken which is only transitive, but not convertible, every syllogism remains valid, provided that the *correlative* of that copula be used instead of it, when needful. And in this consists, according to Mr. De Morgan, the root of the doctrine of *figure*. If + represent affirmative, and — negative, the four figures are connected with + +, + —, — +, and — — (in the system of contraries, where negative premises may have a valid conclusion, the fourth figure has equal claims with the rest, though the conditions of all the figures are singularly altered). These forms do not require the correlative copula: thus + — in the second figure (as *Camestres* and *Baroko* among the Aristotelian forms) are as valid when the copula is '*supports*' or '*is greater than*,' as when '*is*' is employed. But in every other case the rule for the proper introduction of the correlative copula is as follows:—The preceding being called the *primitive forms* of the four figures, when *one* premise of a primitive form is altered, the necessity of a correlative copula is thrown upon *the other*; when *both*, upon the conclusion. Thus, the primitive form of the second figure being + —, and *Cesare* showing — +, it is only valid with the copula '*governs*,' by making '*is not governed by*' the copula of the conclusion, as follows:—

No Z governs any Y
Every X governs a Y
Therefore no X *is governed by* any Z.

* So that there can be but one X and one Y, and that X is Y.

By an additional letter (g) introduced into the usual words of syllogism, the places of the correlative copula may be remembered, as in *Barbara*, *Celagrent*, &c.: a g being made to accompany any member of the syllogism in which the correlative copula must be employed.

This theory is applied equally to the Aristotelian system, to Sir William Hamilton's (though not of universal application in the *cumular* form), and to Mr. De Morgan's system of contraries. The extensions required by the use of a merely transitive copula, in the last-mentioned system, are discussed; and mention is made of the *tricopular* system, in which the leading copula and its correlative have an intermediate or middle relation, equally connected with both; as in $> =$ and $<$ of the mathematicians.

The next step is the assertion that it is not necessary that any two of the three copulæ of a syllogism should be the same; all that is requisite is that, in affirmative syllogisms, the copular relation in the conclusion should be compounded of those in the premises. The instrumental part of inference is described by Mr. De Morgan as *the elimination of a term by composition (including resolution) of relations*, which leads to the conclusion that *whenever a negative premise occurs, there is a resolution of a compound relation.* This resolution is shown in a case (among others) of the ordinary copula, in which, however, it would hardly strike the mind more forcibly than would the properties of powers in algebra if every letter represented unity. Mr. De Morgan shows (in an addition) that in some isolated cases of inference which are not reducible to ordinary syllogism, logicians have had recourse to what amounts to composition of relations.

Mr. De Morgan next points out that the copular relation, in affirmative propositions, need not be restricted as applying to one instance only of the predicate; and shows that the removal of this usual restriction entirely removes *all* his objections to Sir William Hamilton's form of his own system.

Section VI. *On the application of the theory of probabilities to some questions of evidence.*—This inquiry was suggested by the apparent (but only apparent) error of the logicians, who seem to lean towards the maxim that, when the subject and predicate are unknown, the universal and particular propositions 'Every X is Y,' 'Some Xs are not Ys,' are *à priori* of equal probability. The difficulty is one which occurs in the following case:—If a good witness, drawing a card from a pack, were to announce the seven of spades, his credit would not be lowered, though he would have asserted an event against which it was 51 to 1 *à priori*. A common person gives the true answer, 'Why not the seven of spades as well as any other?' Many readers of works on probability would be inclined to say 'That is not the question; why the seven of spades rather than some one or another of the fifty-one others?' The retort is fallacious: it rubs out the distinctive marks from the other fifty-one cards, and writes on each of them 'not the seven of spades' as its only exponent. Laplace has chosen two problems, in one of which the distinctive marks exist, and not in the other; and, neglecting the consideration of the first one, has founded his remarks upon the deterioration of evidence

by the assertion of an improbable event, entirely upon the second. The object of this section is, by a closer examination of the mathematical problem of evidence, to ascertain the accordance or non-accordance of the results of usual data with usual notions. The result of the examination is, that common notions, as in other cases, are found closely accordant with theory. For instance, if there be n possible things which can happen, so that the *mean* probability of an event is $\frac{1}{n}$, a witness of whom we know no *particular bias* towards one mode of error rather than another, asserting an event of which the *à priori* probability is a, has his previous credit raised, unaltered, or lowered, according as $a-\frac{1}{n}$ is positive, nothing, or negative. So that though the *à priori* probabilities were distributed among a million of possible and distinguishable cases, yet a witness asserting one of them against which it is only 999,999 to 1, would have as good a right to be believed as though there had been but two equally probable cases, of which he had asserted one.

March 11, 1850.

On the Numerical Calculation of a class of Definite Integrals and Infinite Series. By Professor Stokes.

In a paper "On the Intensity of Light in the neighbourhood of a Caustic," printed in the sixth volume of the Cambridge Philosophical Transactions, Mr. Airy, the Astronomer Royal, has been led to consider the integral

$$W=\int_0^\infty \cos\frac{\pi}{2}(w^3-mw)dw,$$

and has tabulated it from $m=-4$ to $m=+4$ by the method of quadratures. In a supplement to the same paper, printed in the fifth part of the eighth volume, Mr. Airy has extended the table as far as $m=\pm 5\cdot 6$, by means of a series proceeding according to ascending powers of m. This series, though convergent for all values of m, however great, is extremely inconvenient for numerical calculation when m is large, and moreover gives no information as to the law of the progress of the function for large values of m. The author has obtained the following expression for the calculation of W for large, or even moderately large, positive values of m:

$$W=2\,(3m)^{-\frac{1}{2}}\left\{R\cos\left(\phi-\frac{\pi}{4}\right)+S\sin\left(\phi-\frac{\pi}{4}\right)\right\},$$

where

$$R=1-\frac{1.5.7.11}{1.2(72\phi)^2}+\frac{1.5.7.11.13.17.19.23}{1.2.3.4(72\phi)^4}-\ldots,$$

$$S=\frac{1.5}{1.72\phi}-\frac{1.5.7.11.13.17}{1.2.3(72\phi)^3}+\ldots,$$

$$\phi=\pi\left(\frac{m}{3}\right)^{\frac{3}{2}}.$$

When m is negative, and $+mw$ is written for $-mw$ in the integra

W, so that in the altered form of the integral m is positive, there results

$$W = 2^{-\frac{1}{2}}(3m)^{-\frac{1}{4}}\varepsilon^{-\phi}\left\{1 - \frac{1.5}{1.72\phi} + \frac{1.5.7.11}{1.2(72\phi)^2} - \dots\right\}.$$

By means of these expressions, W may be calculated with great facility when m is at all large. The author has given a table of the roots of the equation $W=0$, from the second to the fiftieth inclusively, calculated by a formula derived from the former of the above expressions. This formula was not sufficiently convergent to give the first root to more than three places of decimals; but this root may be obtained more accurately from Mr. Airy's table.

The method by which the author has treated the integral W appears to be of very general application, and he has further exemplified it by applying it to the infinite series

$$1 - \frac{x^2}{2^2} + \frac{x^4}{2^2.4^2} - \frac{x^6}{2^2.4^2.6^2} + \dots = \frac{2}{\pi}\int_0^{\frac{\pi}{2}} \cos(x\cos\theta)d\theta,$$

which occurs in a great many physical investigations, as well as to the integral which occurs in investigating the diffraction produced by a screen with a small circular aperture, placed in front of the object-glass of a telescope through which a luminous point is viewed.

Curvature of Imperfectly Elastic Beams. By Homersham Cox, B.A. Jesus College.

The equation to the curve of an elastic deflected beam is usually deduced from the assumption,—1, that the longitudinal compression or extension of an elastic filament is proportional to the compressing or extending force; 2, that for equal extension and compression the compressing and extending forces are equal to each other.

These hypotheses are not quite correct in practice. All substances appear to be subject to a *defect of elasticity, i. e.* their elastic forces of restitution increase in a somewhat less degree than in proportion to the extension or compression. If the forces be taken as functions of the latter quantities expressed by a converging series of their ascending integral powers, the terms after the third may in general be neglected as of inconsiderable magnitude. If then e be the extension of a uniform rod of a unit of length and a unit of sectional area, the longitudinal force producing that extension is

$$\alpha e + \beta e^2 + \beta' e^3,$$

where α, β, β' are empirical constants.

Similarly, if c be the compression of a similar rod, the force producing that compression is

$$\gamma c + \delta c^2 + \delta' c^3,$$

where γ, δ, δ' are three other empirical constants.

These formulæ are to be applied to a uniform beam of rectangular section, resting on horizontal supports and slightly deflected at its centre. For this purpose, the compression and extension of every filament of the beam are expressed in terms of the radius of curvature and the distance from the neutral axis. Analytical expressions are thus obtained for the elastic forces developed in any transverse section of the beam; and the position of the neutral axis is obtained

from the integrals of these expressions by the principle, that the sum of all the horizontal forces above is equal to the sum of all the horizontal forces below the neutral axis.

Next, the sums of the *moments* of the elastic forces about the neutral axis are obtained; and the sums are equated to the moment about that axis of the pressure (P) of the fulcrum, the latter moment being the product of half the deflecting weight by the distances (x) of the fulcrum from the point of the neutral axis here considered. This equation involves the radius of curvature, and is solved with respect to the reciprocal of that quantity. It is to be observed, that this equation, and also the preceding one determining the neutral axis, are not of such a form as to admit of direct solution, and are therefore solved by an ordinary method of approximation.

The reciprocal of the radius of curvature of a point (x, y) of a curve is equal to (the second differential of y with respect to x) ÷ (a quantity which becomes equal to unity when, as here, the inclination to the axis of x of the tangent at any point of the curve is comparatively very small).

Making the substitution indicated, and integrating twice the equation last obtained, we obtained finally for the equation to the neutral line of a rectangular beam of vertical depth d, and horizontal breadth μ, and length 2α,

$$y=\frac{\varkappa x^3}{2.3}-\frac{b\varkappa^2x^4}{3.4}+\frac{(2b^2-c)\varkappa^3x^5}{4.5}-\left(\frac{\varkappa a^2}{2}-\frac{b\varkappa^2a^3}{3}+\frac{(2b^2-c)a^4\varkappa^3}{4}\right)x,$$

where

$$b=\frac{3}{4}d(\beta+\delta\alpha^2\gamma^{-2})(1+\alpha^{\frac{1}{2}}\gamma^{-\frac{1}{2}})^{-2}$$

$$c=\frac{3}{5}\frac{d^2}{\alpha}(1+\alpha^{\frac{1}{2}}\gamma^{-\frac{1}{2}})^{-3}(\beta'+\delta'\alpha^{\frac{5}{2}}\gamma^{-\frac{5}{2}})$$

$$\varkappa=\frac{P}{\mu}\frac{3}{d^3\alpha}(1+\alpha^{\frac{1}{2}}\gamma^{-\frac{1}{2}})^2.$$

If, according to the ordinary hypotheses of perfect elasticity, we put $\alpha=\gamma$ and neglect terms depending on β, β', δ, δ', this equation to the elastic curve coincides with that given by Poisson and others.

If we put $x=a$, the value of the deflection at the centre of the beam is

$$\frac{\varkappa a^3}{3}-\frac{b\varkappa^2a^4}{4}+\frac{(2b^2-c)\varkappa^3a^5}{5}.$$

Whence it may be seen that the deflection is greater than it would be if the elasticity were perfect.

On the Knowledge of Body and Space. By H. Wedgwood, M.A.

No part of the great metaphysical problem chalked out by Locke has been more assiduously laboured, and none has attained a less satisfactory solution, than that which relates to the origin of the idea of space and its subordinate conceptions, figure, position, magnitude.

It was seen that the exercise of the muscular frame must somehow be instrumental in making us acquainted with the material and ex-

tended world, but all hopes of a logical explanation of the process by which that effect is produced seemed cut off at the outset by a preliminary objection. The knowledge of motion, it was said, obviously involves the knowledge of the body moved. The consciousness of the motion of the hand therefore implies the conception of the hand itself, an object of certain shape and size. The attempt to account for the notions of shape and size from the motion of the hand was thus apparently stranded in a hopeless paralogism; and so insurmountable was the difficulty taken to be, that philosophers were driven to imagine a second source of elementary ideas, in addition to the simple apprehension of the thing conceived in actual existence, maintaining that space is known to us as the *condition* under which we perceive external things, or. as others express it, that the notion of space arises in the mind on the first apprehension of body by a principle of necessary judgement, which impresses upon us the conviction that all body is contained in space.

In the paper laid before the Society, an attempt is made to show the utter barrenness of this hypothesis of a necessary origin (as it is called) of the idea of space; and the main object of the paper is to rest the idea on a more solid foundation, by showing the adequacy of muscular exertion, in conjunction with the sense of touch, to furnish us with complete knowledge of the material and extended world by the ordinary way of actual experience.

There are two kinds of action; one *instinctive*, immediately induced by the physical constitution of the agent independent of the understanding; the other *rational*, induced by the discernment of some object of desire in the end to be accomplished, and of course implying a previous conception of the action in question.

Familiar instances of instinctive action are then pointed out, from whence it would appear that the sensations of touch felt on contact of any part of the living frame with a foreign body operate as motives to instinctive exertion through the instrumentality of that part of the muscular frame on which the sensible impression is made, instinctively impelling the sentient being to muscular reaction against the material cause of the sensation, or leading him to shrink from it if the sensation is of a painful nature.

Attention is directed in particular to the action of an infant instinctively closing his hand upon a finger placed within his palm; and it is argued that the effect of such an action on his understanding will be the direct apprehension of *body*, a complex object consisting of *surface* (undeveloped as yet in form and magnitude) apprehensible by tactual sensation; and *substance*, revealed by resistance to muscular exertion, constituting a new kind of being essentially different from any of those discerned by means of the five senses.

The relation between body and space is illustrated by comparison with the case of light and darkness, the second of the two correlatives belonging in each case to Locke's class of *positive ideas from negative causes*. As he who has once apprehended light is subsequently enabled to look for that phænomenon in a direction from whence no rays actually penetrate the eye, so, it is argued, will he who has once made use of his hand in the apprehension of body be

enabled to stretch out the same member in feeling for body when none is actually within reach; and as in the former case the failure of the effort to discover light results in the sensible impression of black or darkness, so in the latter case the effort unsuccessfully aimed at the apprehension of body will take effect on the intelligence in the direct cognition or actual experience of space, viz. of that particular portion of space through which the hand is moved in the unsuccessful search after body.

Thus the notion of space, like that of body, or of any sensible phænomenon, is traced to the actual experience of the thing itself in concrete existence. The subsequent enlargement of the idea, so as to comprehend the space occupied by the solid substance of bodies and that which stretches away to infinity in all directions around us, is duly accounted for on the same principle; and that impossibility of conceiving the destruction of any portion of space, on which so much stress has been laid as establishing the necessity of a deeper-seated origin than simple experience, is shown to be the natural consequence of the negative foundation of the idea as explained by the analogy of light and darkness.

April 15, 1850.

On the Mathematical Exposition of some Doctrines of Political Economy. By the Master of Trinity.

The object of this paper was to solve algebraically certain problems which have been solved by Mr. J. S Mill and others by means of numbers, taken as examples; the principles of these writers being taken for granted in the algebraical solution. Mr. Mill has rightly observed, that instead of saying that prices are determined by the *ratio* of demand and supply, we ought to say that they are determined by the *equation* of demand and supply. This equation may be thus stated. Let p be the price, and q the quantity bought and sold at that price. When p becomes p', let q become q'; and p' being equal to $p(1+n)$, let $p'q'=pq(1+mn)$: this is the equation of demand and supply. For different commodities, we have different values. There are such classes of commodities as these: (A.) *Conventional necessaries*, for which $m=1$: of these the same quantity is bought whatever be the price. (B.) *Articles of fixed expenditure*, for which $m=0$: on these the same sum is always expended, a smaller quantity being bought in proportion as they are dearer. (C.) *Common necessaries*, in which m is between 1 and 0: in these, when the price falls, the consumption is increased, but the money expended diminished. (D.) *Popular luxuries*, in which m is negative: in these, when the price falls, the consumption is so much increased that the money expended on them is increased also. For corn, the mean value of m seems to be about $\frac{1}{2}$: on this supposition a failure of one-fourth in the supply would double the price. The quantity m measures the susceptibility of the price to change when the supply changes, and also the intensity of the demand.

Another division of commodities is, according to the cost of production. These are (α) commodities of fixed and limited supply; (β) commodities of fixed cost; (γ) commodities of increasing cost for increasing supply, as for instance corn in a given limited district. The equation of price for the last case was given.

The like methods were applied to solve certain problems concerning international trade, treated by Mr. Mill. If the relative value of two commodities, C and D, in England and Germany be different, there will be a saving in exporting each from where it is cheaper to where it is dearer; and the question is, at what point prices will settle. We must introduce here the principle of *the uniformity of international prices*; namely, that when the trade is established, the relative prices of C and D will be the same in the two countries: the principle of the *equality of imports and exports* in each country; and the *equation of demand and supply* already stated. By combining these principles, the problem of the resulting price is solved. But it is found that there is no solution possible (that is, no solution in which both countries gain by the trade), except the *mutual demand* for the interchange of commodities be nearly equal. This limitation of the solution is given by the algebraical method, and seems to have been overlooked by previous writers.

The same methods were extended to a greater number of exported and imported commodities; and finally, it was remarked that these calculations are all founded on principles of equilibrium, whereas a state of equilibrium is never attained; and thus the theory may be very imperfectly applicable, like the equilibrium theory of the tides.

Second Memoir on the Intrinsic Equation of Curves. By the Master of Trinity.

The intrinsic equation of curves, according to which any curve is expressed by means of an equation between its length (s) and its angle of deflection (ϕ), may be conveniently used for many purposes. When a curve is so represented, the portion of the length which comes after a cusp must necessarily be taken as negative. This had appeared anomalous to some mathematicians, on the ground that a cusp is in all cases the limit of a loop. To clear up this point, the author adduces two cases. (1.) The curve of which the equation is $s = a\phi + b \sin \phi$, which is a looped curve when b is less than a, and a cusped curve otherwise. But in this curve it appears that a loop arises from the vanishing of *two* cusps, and of the intervening negative portion of the arc. (2.) The case of the ordinary trochoid, which is a looped curve when the describing point is exterior to the rolling circle, and becomes a cusped curve (a cycloid) when the point is in the circle. But in this case the length of the trochoid is equal to the length of an elliptical arc, which, in the case of the cycloid, coincides with the major axis, and becomes negative beyond the vertex of the ellipse. Other equations were examined, which give *running pattern* curves with cusps, cusped curves with infinite diverging spirals at the extremities, and sinuous curves with infinite converging spirals at the extremities; and certain integrals which occurred in the former memoir on this subject were discussed.

May 13, 1850.

Results* connected with the theory of the singular solution of a Differential Equation of the first order between two variables. By Professor De Morgan.

By a singular solution of a differential equation is here meant any solution which can be obtained by differentiation only, whether it be a case of the primitive by integration or not.

By a curve is meant all that is included under one equation, whether resoluble into what are commonly called complete curves or not. Thus, the equation

$$(x-y)(x^2+y^2-1)=0$$

belongs to *a curve*, having a rectilinear branch and a circular one.

By such a symbol as v_x is meant the partial differential coefficient $\frac{dv}{dx}$, when obtained from an equation in which v is explicitly expressed in terms of x and (it may be) other variables.

Let $\varphi(x, y, c)=0$ be the complete primitive of the differential equation $y'=\chi(x, y)$.

$\varphi(x, y, c)$ belongs to two distinct classes of curves:—

1. Continuous curves derived from such values of c, real or imaginary, as will enable $\varphi=0$ to exist for points infinitely near to one another.

2. Systems of points, derived from

$$A(x, y, \alpha, \beta)=0, \quad B(x, y, \alpha, \beta)=0,$$

where

$$\varphi(x, y, \alpha+\beta\sqrt{-1})=A(x, y, \alpha, \beta)+B(x, y, \alpha, \beta).\sqrt{-1}.$$

When a curve is such that the points on one side of it are on curves of the first kind, and those on the other side are part of systems of the second kind, let that curve be called a *separator*; and the same when it separates points of both kinds from points which belong to one kind only.

No solution of the differential equation can be formed by combining all those systems of the second kind in which α and β are connected by a real relation.

The curve which has at every point of it, either

$$\frac{\varphi_x}{\varphi_c}=\infty, \quad \frac{\varphi_y}{\varphi_c}=\infty,$$

or

$$\frac{\varphi_x}{\varphi_c}=\infty, \quad \frac{\varphi_y}{\varphi_c} \text{ finite}, \quad x=\text{const.},$$

* This communication is the abstract of a part of a paper not yet completed, and was forwarded to the Society for the purpose of ascertaining whether any examples could be produced destructive of the perfect generality of the results.

or $$\frac{\varphi_y}{\varphi_c}=\infty, \quad \frac{\varphi_x}{\varphi_c} \text{ finite}, \quad y=\text{const.},$$
is a singular solution. And in the above are contained *all* the singular solutions.

Every branch of a singular solution is either—

A separator, only.

A curve, every point of which has a contact of the first order at least with some one real primitive, only. Or both. Or neither.

If the first or last, it is a case of the complete primitive. And such cases may be introduced at pleasure into the singular solution, by writing the primitive in the form
$$\varphi(x, y, fc)=0.$$

A branch of a singular solution has at the utmost n contacts with each primitive which it touches (n being determined by the nature of the equation), and all of the first order, generally. Or, p_1 of the first order, p_2 of the second, &c., $p_1+2p_2+3p_3+\ldots$ being n or $n-$ (an even number); or some of these cases for some primitives, others for others, including the possibility of some cases giving none at all, when n is even.

The branches of the singular solution which have contact with ordinary primitives (whether themselves ordinary primitives or not) to the exclusion of the branches which are only separators, may be determined from the differential equation by the following test.

Let $y'=\chi(x, y)$ be the differential equation; whence
$$y''=\chi_x+\chi_y\cdot\chi.$$

Find the curves which satisfy either of the following sets of conditions:—
$$\chi_x=\infty \qquad \chi_y=\infty \qquad y'' \text{ finite},$$
or
$$\chi_x=\infty \qquad x=\text{const.} \quad y'' \text{ finite},$$
or
$$\chi_y=\infty \qquad y=\text{const.} \quad y'' \text{ finite}.$$

Every such curve *does* satisfy the differential equation, and is a *singular solution* having contact with some one primitive at every point.

And other such singular solutions there are none except those designated by $x=\infty$, or $y=\infty$, or both.

But if
$$\chi_x=\infty \text{ and } \chi_y=\infty \qquad y''=\infty,$$
or
$$\chi_x=\infty \qquad x=\text{const.} \quad y''=\infty,$$
or
$$\chi_y=\infty \qquad y=\text{const.} \quad y''=\infty,$$
then the differential equation may or may not be satisfied; but the curve passes through the singular points of the primitives, with or without contact, according as the differential equation is or is not satisfied. An evolute is such a pseudo-singular solution to all the involutes, passing through their cusps.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 11, 1850.

On the Mathematical exposition of certain Doctrines of Political Economy. By the Master of Trinity. Third Memoir.

The object of this memoir was to point out some of the laws of international trade, taking into account the effect of the import or export of money, *i. e.* of metallic currency. It was stated that when the balance of imports and exports is deranged by the import of a new commodity, previously produced at home, the effect is, to diminish the annual import of gold and silver; hence, to lower the scale of prices in general; hence, to increase the exports, and thus, to produce a new condition of equilibrium: and the necessary suppositions being made, the amount of depression in prices arising from such a cause was calculated.

The Master of Trinity also made a communication relative to *a new kind of coloured fringes*. He stated that he had, many years ago, remarked that if we hold a candle before a *dusty* looking-glass at a distance of six or eight feet, so that the image of the candle is near to that of the eye, the image of the candle is seen in the middle of a patch of coloured bars, which are perpendicular to the plane passing through the candle and the eye, normal to the looking-glass. This remark was communicated to M. Quetelet, and published by him. Attention has recently been drawn to this observation, at the Congress of Swiss men of science, held at Aarau, in August of the present year. M. Mousson of Zurich pointed out, at that meeting, the differences between the stripes noticed by Dr. Whewell, and the rings on specula observed by Fraunhofer. Among these differences are,—1st, Fraunhofer's rings depend upon the first surface of the speculum, the stripes upon both; 2nd, the rings are not produced except the dust be particles of uniform size; the stripes are produced by dust of irregular and various particles; 3rd, the rings depend for their size on the size of the particles of dust; the stripes do not.

Some discussion took place as to the manner in which these stripes arise from the theory of interferences, and upon their relations to Newton's "colours of *thick* plates."

December 9, 1850.

On the effect of the internal friction of Fluids on the Motion of Pendulums. By Professor Stokes.

It has been acknowledged for some time that the results which follow from the common theory of fluid motion relative to the effect of a fluid on the time of vibration of a pendulum do not agree well with observation. The volume of the Philosophical Transactions for 1832 contains the results obtained experimentally by the late Mr. Baily relating to the effect of air in altering the time of vibration of a great variety of pendulums. The experimental results are exhibited by the value of n, the factor by which the correction for buoyancy must be multiplied in order to give the whole effect observed. With pendulums composed of spheres suspended by fine wires, Baily found $n=1{\cdot}864$ for spheres a little less than $1\frac{1}{2}$ inch in diameter, and $n=1{\cdot}748$ for spheres about 2 inches in diameter. The result which follows from the common theory is $n=1{\cdot}5$, as was first shown by Poisson. The value $1{\cdot}864$ was the mean of 16 pair of experiments, giving a mean error $0{\cdot}023$, and $1{\cdot}748$ was the mean of 12 pair, which gave a mean error $0{\cdot}014$, so that the difference between the two results, and between either of them and the common theory, is far too large to be attributed to errors of observation.

The chief object of this paper was, to apply to the calculation of the motion of a pendulum the general equations of motion which are arrived at when the internal friction of the fluid is taken into account, and to compare the resulting formulæ with the experiments of Baily and others. The general equations, simplified, *first*, by neglecting the square of the velocity, *secondly*, by neglecting the compressibility of the fluid, the effect of which in the present instance is in fact quite insignificant, *thirdly*, by omitting the external forces, the effect of which may be taken into account separately, are

$$\frac{1}{\rho}\frac{dp}{dx}=\mu'\left(\frac{d^2u}{dx^2}+\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}\right)-\frac{du}{dt}, \text{ \&c.} \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

The second and third of the general equations are not written down, because they may be supplied by symmetry. In these equations ρ is the density, p the mean of the normal pressures in the direction of any three rectangular planes passing through the point of which x, y, z are the coordinates; u is the velocity in the direction of x, t the time, and μ' a certain constant, depending upon the nature of the fluid, which the author proposes to call the *index of friction*.

The author has succeeded in obtaining the solution of equations (1.) in the two cases of a sphere and of an infinite cylinder. The latter may be applied to the case of a pendulum consisting of a long cylindrical rod, by treating each element of the rod as belonging to an infinite cylinder oscillating with the same linear velocity. The following is the solution in the case of a sphere, so far as relates to the resultant action of the fluid on the sphere.

Let ξ be the abscissa of the centre of the sphere, measured in the

direction of the motion, a the radius of the sphere, τ the time of vibration, M′ the mass of the fluid displaced, F the resultant force of the fluid on the sphere, so that $-$F is the resistance; then

$$-\mathrm{F}=k\mathrm{M}'\frac{d^2\xi}{dt^2}+k'\frac{\pi}{\tau}\mathrm{M}'\frac{d\xi}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

where

$$k=\frac{1}{2}+\frac{9}{2}\left(\frac{\mu'\tau}{2\pi a^2}\right)^{\frac{1}{2}}, \qquad k'=\frac{9}{2}\left(\frac{\mu'\tau}{2\pi a^2}\right)^{\frac{1}{2}}+9\,\frac{\mu'\tau}{2\pi a^2}. \quad . \quad . \quad (3.)$$

The effect of a fluid on the time of vibration depends on the term which involves k; the effect on the arc of vibration depends on the term which involves k'.

The expression for F has precisely the same form (2.) in the case of a cylinder, but k and k' are certain transcendental functions of $(\mu'\tau)^{\frac{1}{2}}a^{-1}$ (a here denoting the radius of the cylinder), which the author has tabulated.

The value of μ' having been determined for air, or any given fluid, by one experiment giving the effect of the fluid either on the time of vibration, or on the arc of vibration, of any one pendulum consisting either of a sphere suspended by a fine wire, or of a long cylindrical rod, or of a combination of a sphere and a rod, the formulæ which follow from (2.) ought to make known the effect of the fluid both on the time and on the arc of vibration of all pendulums of the above forms. The agreement of theory with the experiments of Baily relating to the effect of the air on the time of vibration of pendulums is remarkably close. Even the rate of diminution of the arc of vibration, the observation of which held quite a subordinate place in Baily's experiments, agreed with the rate calculated from theory as closely as could reasonably have been expected.

The value of the index of friction of water was deduced by the author from some experiments of Coulomb's on the decrement of the arc of oscillation of discs which performed extremely slow oscillations in their own plane by the force of torsion. When this value was substituted in the expression for the time of vibration of a sphere, the result was found to agree almost exactly with Bessel's experiments on the time of vibration of a sphere swung in water.

As a limiting case of the problem of a ball pendulum, the author has deduced the resistance of a fluid to a sphere moving uniformly under such circumstances that the square of the velocity may be neglected. The resistance thus determined proves to be proportional, not to the surface, but to the radius of the sphere; and therefore the quotient of the resistance divided by the mass increases very rapidly as the radius decreases. Accordingly, the terminal velocity of a minute globule of water descending through the air depends almost wholly on the internal friction of air. Since the index of friction is known from Baily's pendulum experiments, the terminal velocity can be calculated numerically for a globule of given diameter. The velocity thus calculated proves to be so small, in the case of globules

such as those of which we may conceive the clouds to be formed, that the suspension of the clouds does not seem to offer any difficulty. Had the pressure been strictly equal in all directions in air in the state of motion, the terminal velocity of such globules would have been far larger, and consequently the quantity of water which could have existed in the air in the state of cloud would have been immensely diminished. It appears therefore that these small and hitherto almost unrecognized forces, which depend on internal friction, are essential to the fertility of at least the tropical regions of the earth.

The author has also applied the theory of internal friction to the calculation of the subsidence of a series of oscillatory waves. On substituting for the index of friction in the resulting formula the numerical value deduced from the experiments of Coulomb, it appears that in the long swell of the ocean the effect of friction is insignificant, whereas in the case of the short ripples excited on a small pool by a puff of wind the subsidence due to friction is very rapid. Accordingly, short ripples of this kind quickly die away when the breeze that excited them ceases to blow.

February 24, 1851.

On some points of the Integral Calculus. By Professor De Morgan.

Some time ago, Mr. De Morgan communicated to the Society an abstract of some unfinished views on the connexion between the ordinary and singular solution of a differential equation. The present paper completes those views, and also contains sections on the solution of differential equations by elimination, on the proof of the number of constants which a solution may contain, and on the criterion of integrability of a function of x, y, and differential coefficients of y.

1. *On singular solutions.*—As to equations of the first order, the tests obtained in this paper may be described as follows:—

Mr. De Morgan means by a *singular* solution any one which is obtained by other process than integration, whether it be contained in the integrated primitive, or not. When the singular solution is *not* contained in the primitive, he calls it an *extraneous* solution.

Let $\phi(x, y, c)=0$ be the primitive equation, giving $c=\Phi(x, y)$. The differential equation then is

$$y'=-\frac{\Phi_x}{\Phi_y} \text{ say} = \chi(x, y) \qquad \left\{ \Phi_x=\frac{d\Phi}{dx}, \text{ \&c.} \right\}$$

$$\text{and } \Phi_x=-\frac{\phi_x}{\phi_c} \qquad \Phi_y=-\frac{\phi_y}{\phi_c}$$

Every relation between x and y which satisfies either of the fol-

lowing collective conditions, *is a solution of the equation*; and, by definition, a singular solution.

1. Φ_x and Φ_y both infinite.
2. Φ_x *only* infinite, and $x=$const.
3. Φ_y *only* infinite, and $y=$const.

And all possible finite solutions of the differential equation are given either by the original primitive, or by these relations.

Let $X=\frac{\chi_x+\chi_y\chi}{\chi_y}$. Then all relations between x and y which satisfy either of the following collective conditions are solutions of the differential equation, and are singular solutions.

1. χ_x and χ_y both infinite, and $X=0$.
2. χ_x only infinite, $x=$const., and $X=0$.
3. χ_y only infinite, $y=$const., and $X=0$.

But when one of these sets fails only in that X does not vanish, the curve so obtained, instead of having contact with a primitive curve at every one of its points, passes through the points of infinite curvature of the primitives; and the differential equation which is satisfied is $y'=\chi-X$. Every evolute is related in this manner to its involutes, passing through all their cusps.

The above tests do not give the possible case in which $x=\infty$, or $y=\infty$, is a singular solution.

Mr. De Morgan proposes the following geometrical illustration of the connexion between the primaries and the singular. Let c be the third ordinate of a surface (usually denoted by z) having the equation $\phi(x, y, c)=0$. The projections upon the plane of xy of sections parallel to that plane are the primaries: the singular solution is the base, upon the plane of xy, of a cylinder perpendicular to that plane, and which always touches the surface. By means of this illustration, it may be made manifest that certain cases of singular solution which have always been discarded as unmeaning, are limiting cases of the kind which are admitted in analysis so soon as the way up to the limit is clearly seen.

Taking a general equation with two arbitrary constants, so that a relation between those constants selects and designates a family of curves, it is shown generally (without examination of exceptional cases) how to find the families which have with their singular curves contact of the *second* order. The equation of these singular curves is a differential equation of the first order: but *its* singular solution is the singular curve of a family of curves which have with it a contact of the *third* order.

2. *Solution of differential equations by elimination.*—This is an idea derived from the method which Mr. De Morgan communicated (vol. viii. part 5) relative to partial differential equations, and which he found, after his paper was finished, had been given by M. Chasles, as he supposed, from knowledge of the *results* of Monge. But it afterwards appeared that the authority for Monge having obtained

such results is only a candid supposition *of M. Chasles himself*, and that no memoir on the subject, written by Monge, has been traced. All that M. Chasles had to proceed on was the *title* of a memoir mentioning a certain mode of generating conjugate surfaces, from which he thought it very likely that the solution of partial differential equations which he himself thence found, had really been found by Monge. Under these circumstances, Mr. De Morgan is of opinion that the method must be attributed to M. Chasles as its first discoverer, at least until something further appears.

Mr. De Morgan proceeds to make use of the equation

$$\int P.dy^{(n)} = Py^{(n)} - P'y^{(n-1)} + \ldots$$

to form various cases of equations which can be reduced to lower orders, and which can finally be solved by elimination. Of these, the most simple specimen, being the one suggested by thinking on the method above alluded to, is as follows:—

If $x = Y'$ and $y = XY' - Y$, Y being a function of X, whence y is a function of x, we have the following sets of correlative equations:—

$$\begin{array}{ll} x = Y' & X = y' \\ y = XY' - Y & Y = xy' - y \\ y' = X & Y' = x \\ y'' = \dfrac{1}{Y''} & Y'' = \dfrac{1}{y''} \\ y''' = -\dfrac{Y'''}{Y''^3} & Y''' = -\dfrac{y'''}{y''^3} \end{array}$$

and so on. If, then, $\phi(x, y, y', y'', y''', \ldots) = 0$ be a given differential equation, and if it be found that

$$\phi\left(Y', XY' - Y, X, \frac{1}{Y''}, -\frac{Y'''}{Y''^3}, \ldots\right) = 0$$

can be solved; it is seen that the original equation can be solved by eliminating X between $x = Y'$ and $y = XY' - Y$.

The general method of which this is a particular case, is as follows. Let $f(x, y, X, Y) = 0$ have its differential equations of the first order formed on two suppositions: first, that X and Y are constant; secondly, that x and y are constant. Let these differential equations be

$$\begin{array}{ll} X = \Phi(x, y, y') & x = \phi(X, Y, Y') \\ Y = \Psi(x, y, y') & y = \psi(X, Y, Y'). \end{array}$$

These equations may be used instead of the first two pairs of correlatives in the preceding example: and each differential coefficient of Y is expressible by means of the same and lower differential coefficients of y; and *vice versâ*. To get convertible forms, as in the instance above, $f(x, y, X, Y)$ must be chosen so that x and y are simultaneously interchangeable with X and Y.

Mr. De Morgan gives a similar extension of the method as applied to partial differential equations.

3. *On the constants of a primitive equation.*—It is usually left to

be collected from induction that the equation of the nth order has n constants, and no more, in its complete primitive. Mr. De Morgan proposes an *à priori* proof of this point, on which, as in all such cases, it would be presumptuous to decide until it has been thoroughly examined.

He further proposes an extension of the meaning of the term *solution*, in the case of all the primitives intermediate between the differential equation and the original primitive. Thus, supposing an equation of the third order, of which the admitted primitives of the second order are

$$U_1 = \text{const.}, \quad U_2 = \text{const.}, \quad U_3 = \text{const.},$$

he maintains that the general primitive of the second order is

$$f(U_1, U_2, U_3) = 0,$$

where f is any function whatsoever: and, starting from this last equation, he determines a general primitive of the first order in a similar way.

This view is supported by the reduction of a common differential equation of the nth order to a partial differential equation of the first order with n independent variables.

4. *On the criterion of integrability of* $\phi(x, y, y', y'', \ldots)$.—If we denote the differential coefficients of y by p, q, r, s, &c., it is well known that the condition which is both necessary and sufficient, in order that $V = \phi(x, y, p, q, \ldots)$ may be integrable without reference to relation between y and x, is

$$V_y - V_p' + V_q'' - V_r''' + \ldots = 0,$$

the accent denoting complete differentiation with respect to x. This has usually been established, either by the calculus of variations, or by a process of elaborate expression of the actual result in terms of definite integration with respect to a subsidiary variable. Mr. De Morgan, after some remarks upon the manner in which certain proofs of the *necessity* of the criterion fail, gives a very simple elementary proof founded upon the following theorem. If U be any function of x, y, p, &c.,—as far say as s, for an instance,—then

$$(U')_x = U_x', \ (U')_y = U_y', \ (U')_p = U_p' + U_y,$$
$$(U')_q = U_q' + U_p, \ (U')_r = U_r' + U_q, \ (U')_s = U_s' + U_r, \ (U')_t = U_s.$$

Mr. De Morgan takes it to have been hitherto unnoticed that the formulæ $V_p - V_q' + V_r'' - \ldots$, $V_q - V_r' + \ldots$, so much used in this subject, are, when V is integrable, nothing but the differential coefficients of $\int V dx$, with respect to y, p, &c.

[But since the paper was communicated, Mr. De Morgan has found the above theorem, and its consequences, in a memoir by M. Sarrus, apparently belonging to the *Journal de l'École Polytechnique*, and printed in 1824. No notice is taken of this method by MM. Bertrand, Binet, or Moigno, who have written on the subject since M. Sarrus.]

May 5, 1851.

Of the Transformation of Hypotheses in the History of Science. By W. Whewell, D.D.

The author remarks that new theories supersede old ones, not only by the succession of generations of men, but also by transformations which the previous theories undergo. Thus the Cartesian hypothesis of vortices was modified so that it explained, or was supposed to explain, a central force: and then, the Cartesian philosophers tried to accommodate this explanation of a central force to the phænomena which the Newtonian principles explained; so that in the end, their theory professed to do all that the Newtonian one did. The machinery of vortices was, however, a bad contrivance to produce a central force; and when it was applied to a globe, its defect became glaring. Still however, the doctrine of vortices has in it nothing which is absurd anterior to observation. The "nebular hypothesis" is a hypothesis of vortices with regard to the origin of the system of the universe, and is now held by eminent philosophers. Nor is the doctrine of the universal gravitation of matter at all inconsistent with some mechanical explanation of such a property; for instance, Le Sage's. We cannot say therefore that if the planets are moved by gravitation, they are not moved by vortices. The Cartesians held that they were moved by both; by the one, because by the other.

Like remarks may be made with respect to the theories of magnetism and of light.

May 19, 1851.

On the Colours of Thick Plates. By G. G. Stokes, M.A., Fellow of Pembroke College, and Lucasian Professor of Mathematics in the University of Cambridge.

By the expression "colours of thick plates" is usually understood the system of coloured rings, discovered by Newton, which are formed on a screen when the sun's light is transmitted through a small hole in the screen, and received perpendicularly upon a concave mirror of quicksilvered glass, placed at such a distance from the screen that the image of the hole is at the same distance from the mirror as the hole itself. The brilliancy of the rings, as was afterwards discovered, is greatly increased by tarnishing the surface of the mirror; and it is also advantageous to use a lens to collect the sun's rays, and to place the screen so that the small hole may be situated at the focus of the lens. These rings were first explained on the undulatory theory by Dr. Young, who attributed them to the interference of two streams of light, of which the first is scattered at the tarnished surface of the mirror, and then regularly reflected and refracted, while the second is regularly refracted and reflected, and then scattered in coming out of the glass. The theory has been worked out in detail

by Sir John Herschel, who has investigated the case in which the two surfaces of the glass belong to a pair of concentric spheres, and the hole in the screen is situated in the common centre of curvature.

A set of coloured bands has since been observed by Dr. Whewell in a common plane mirror. These bands are seen when a candle is held near the eye, at the distance of several feet from the mirror, and is viewed by reflexion. It is necessary that the first surface of the glass should be a little tarnished. The theory of these bands had not been worked out, and it had even been doubted by some philosophers whether they were of the nature of the coloured rings of thick plates.

In this paper the author gave a general investigation, which includes as particular cases the theory of the rings formed on a screen in Newton's experiment, and that of the bands which Dr. Whewell had observed in a plane mirror, and which are not thrown on a screen, but viewed directly by the eye. He also exhibited to the meeting a variation of Newton's experiment, in which an extremely beautiful system of rings is very easily produced without sunlight. The face of a concave mirror of quicksilvered glass was prepared by pouring on it a mixture consisting of one part of milk to three or four of water, and then holding the mirror vertically in front of a fire to dry. When the flame of a taper, or of an oil-lamp with a small wick, is placed in front of a mirror thus prepared, in such a position as to coincide with its inverted image, a beautiful system of rings is seen encompassing the flame. These rings appear to have a definite position in space, like a bodily object. The rings thus formed, which are evidently of the nature of Newton's coloured rings of thick plates, may be made to pass in a perfectly continuous manner into the coloured bands observed by Dr. Whewell.

The author has compared theory and experiment in various particulars, and has found the agreement perfect. It will be sufficient to mention here one result of theory, which is of great generality and of considerable elegance. It applies to the system of rings seen by reflexion in a mirror, either plane or curved, when a luminous point is placed anywhere near the axis, and the eye occupies any other position likewise near the axis. The result is as follows:—Join the eye with the luminous point, and likewise with its image, whether it be real or virtual, and find the points in which the joining lines, produced if necessary, cut the mirror. Describe a circle having for diameter the line joining these two points. This circle will be the middle line of the bright colourless fringe of the order zero, and on each side of it the colours will be arranged in descending order.

June 2, 1851.

On a new Elliptic Analyser. By Professor Stokes.

After mentioning some of the inconveniences and inaccuracies attending the use of a Fresnel's rhomb in the analysis of elliptically-polarized light, and alluding to some other methods which had been

employed for the purpose, the author proceeded to describe a new instrument which he had invented, and which he exhibited to the meeting. In the construction of this instrument he had aimed at being independent of the instrument-maker in all important points except the graduation. The construction is as follows :—

A brass rim or annulus is mounted so as to stand with its plane vertical when placed on a table. Within this rim turns a brass graduated disc; and the angle through which it turns is read off by means of verniers engraved on the face of the rim, and reading to tenths of a degree. This disc is pierced at the centre, and carries on the side turned towards the incident light a retarding plate of selenite, of such a thickness as to give a difference of retardation in the oppositely polarized pencils amounting to *about* a quarter of an undulation. In front it carries a hollow cylinder, turned on the lathe along with the disc itself. Round this cylinder there turns a collar containing a Nicol's prism, and carrying a pair of bevel-edged verniers, by which the angle may be read off through which the prism has been turned. Thus the retarding plate moves in azimuth carrying the prism along with it, and the prism has likewise an independent motion in azimuth.

In observing, the light is extinguished by a combination of the two movements, in which case the elliptically-polarized light is converted by the retarding plate into plane polarized, which is then extinguished by the Nicol's prism. On account of chromatic variations, the light is not, strictly speaking, extinguished, unless homogeneous light be employed, but only reduced to a minimum. There are two principal positions of the retarding plate and Nicol's prism in which the light is extinguished, or at least would be extinguished if the incident light were homogeneous; and for each principal position there are four subordinate positions, since either the retarding plate or the Nicol's prism may be reversed by turning it through 180°. The mean of the four subordinate positions may be taken for greater accuracy.

Let R, R′ be the readings of the fixed, r, r' those of the moveable verniers in the two principal positions; I the index error of the fixed verniers, that is, the azimuth of the major axis of the ellipse described, measured from a plane fixed in the disc; i the index error of the moveable verniers, that is, the azimuth of the principal plane of the prism, measured from a fixed plane in the disc; ϖ the angle whose tangent is equal to the ratio of the axes of the ellipse described; ρ the difference of retardation of the oppositely polarized pencils transmitted through the plate, measured as an angle, at the rate of 360° to one undulation. Then the unknown quantities I, i, ϖ, and ρ are given in terms of the known quantities R, R′, r, and r' by the following formulæ, which happen to be extremely convenient for numerical calculation :—

$$\mathrm{I}=\frac{1}{2}(\mathrm{R}'+\mathrm{R})\,; \qquad i=\frac{1}{2}(r'+r)\,;$$

$$\cos 2\varpi=\frac{\sin(r'-r)}{\sin(\mathrm{R}'-\mathrm{R})}; \qquad \cos\rho=\frac{\tan(r'-r)}{\tan(\mathrm{R}'-\mathrm{R})}.$$

The author stated that he had already observed with this instrument, and after a little practice had found that it worked in a very satisfactory manner. When the light of the clouds was reflected horizontally by a mirror, and modified so as to produce elliptically-polarized light in which the ratio of the axes was about 3 to 1, it was found that the mean error of single observations amounted to about a quarter of a degree in the determination of the azimuth of the major axis, about three or four thousandths in the determination of the ratio of the minor to the major axis, and little more than the thousandth part of an undulation in the determination of ρ.

Since the magnitude of ρ depends upon the length of wave, or, what comes to the same, the refrangibility of the light, it follows that a knowledge of the former leads to a knowledge of the latter. It may thus be said that the instrument determines the azimuth and excentricity of the ellipse described, and the refrangibility of the light. An error of the thousandth part of an undulation in the determination of ρ would correspond to an error in the place in the spectrum assigned to the light operated on amounting to less than the twentieth part of the interval between the fixed lines D and E. Now by the use of absorbing media it is possible, without too much reducing the intensity of the light employed, to alter greatly its mean refrangibility; and yet for each medium the refrangibility may be determined very accurately by means of the value of ρ. Accordingly, the instrument is specially adapted for investigations relating to the dispersion of metals, and for other similar researches.

PROCEEDINGS

CAMBRIDGE PHILOSOPHICAL SOCIETY.

December 8, 1851.

On the Oscillations of Suspension Bridges. By J. H. Röhrs Esq., M.A.

In this paper the oscillations of a chain suspended at two points were discussed, with a view to explain the causes of fracture in suspension-bridges, by vibration arising from the tramping of troops, gusts of wind, &c., as well as to suggest means for obviating the mischief under those circumstances. The following were some of the most remarkable results arrived at:—

1st. That if the tension at the ends of the chain where it is suspended be kept constant by allowing play at those points, the variation of tension due to vibration at any other point of the chain will be but small.

2ndly. That if the chain be tied at the points of suspension so that it can have no motion there, a slight extent of vibration will produce comparatively a great increase of tension.

3rdly. That periodic forces, such as may be taken, for instance, to represent the effect of tramping in time of troops moving across the bridge, are dangerous in the extreme, as if they happen to coincide in period with any of the possible types of vibration, the extent of vibration will increase continuously, till it ceases to be represented approximately by a linear or even an equation of the second order; in this case, the chain will be divided by nodal points where there is no vertical motion.

4thly. That the mere transit, without tramping, of ordinary loads at an ordinary pace would not cause sensible vibration in a bridge of wide span; but that terms not periodic might be introduced by the variable pressure of wind sweeping in rapid gusts along the platform.

February 16, 1852.

On the Composition and Resolution of Streams of Polarized Light from different Sources. By Professor Stokes.

In this paper the author investigates the nature of the light resulting from the union of several independent streams of polarized light.

The refrangibility of the several streams is supposed to be the same, and the polarization to be of the most general nature, that is, to be elliptic. The following proposition is established.

When any number of independent polarized streams, of given refrangibility, are mixed together, the nature of the mixture is completely determined by the values of four constants, A, B, C, D, defined in the following manner:—Let J be the intensity of one of the elliptically-polarized streams, α the azimuth of its plane of maximum polarization, tan β the ratio of the axes of the ellipse described by the æthereal particles; then

$$A=\Sigma(J)\,;\ B=\Sigma(J\sin 2\beta)\,;\ C=\Sigma(J\cos 2\beta\cos 2\alpha)\,;$$
$$D=\Sigma(J\cos 2\beta\sin 2\alpha).$$

Two groups of polarized streams, of the same refrangibility, which are such as to give the same values to each of the four constants A, B, C, D, are defined to be *equivalent*; and the author has shown, that if two equivalent groups be transmitted through any optical train, and be afterwards analysed, they will present exactly the same appearance; so that equivalent groups may be regarded as optically identical.

It readily follows from the above theorem, that any group of polarized streams is equivalent to a stream of common light combined with a stream of elliptically-polarized light from a different source. If J, J′ be the intensities of these streams, α' the azimuth of the plane of maximum polarization of the latter, tan β' the ratio of the axes of the characteristic ellipse,

$$J=A-\sqrt{(A^2+B^2+C^2)}\,;\ J'=\sqrt{(A^2+B^2+C^2)}\,;$$
$$\sin 2\beta'=\frac{B}{\sqrt{(A^2+B^2+C^2)}}\,;\ \tan 2\alpha'=\frac{D}{C}.$$

The author has applied these formulæ to a few examples, and has likewise shown, from the general principles established in the paper, that the changes which are continually taking place in the epoch and intensity of the vibrations of polarized light may be of any nature. In the case of common light, the author contends that there is no occasion to suppose the transition from a series of vibrations of one kind to a series of another kind to be abrupt, but that it may be of any nature.

Professor Miller made a communication on the Artificial Formation of Crystallized Minerals.

March 1, 1852.

Mr. Hopkins, F.R.S. &c., gave a Lecture on the Influence of Internal Heat, Stellar Radiation, and Configuration of Land and Sea in producing Changes of the Earth's Superficial Temperature.

March 15, 1852.

Professor Miller made a communication on different improvements in the Reflective Goniometer; and a description of a New Reflective Goniometer.

Professor Stokes concluded a paper on the Composition and Resolution of Streams of Polarized Light from different Sources (see the abstract under the date Feb. 16, 1852, Phil. Mag. vol. iii. p. 316).

He also made a communication on Haidinger's Brushes.

Also on the Optical Properties of a New Salt of Quinine. The salt alluded to is that which had recently been discovered by Dr. Herapath (Phil. Mag. vol. iii. p. 161). The substance of this communication formed the subject of a notice of the properties of the salt which the author read at the Meeting of the British Association at Belfast, which will be found in the Report of the Transactions of the Sections.

April 26, 1852.

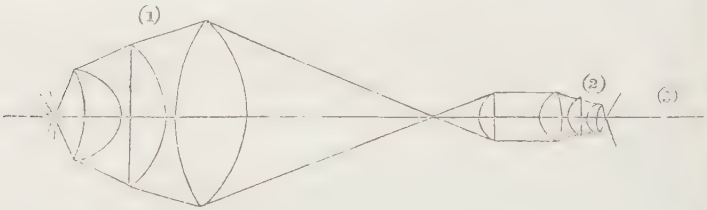
The Rev. Mr. Kingsley gave an account of the application of Photography to the Microscope.

The earliest attempts in photography were directed both by Sir H. Davy and Mr. Fox Talbot to the fixing upon prepared paper the images of objects by the solar microscope, and the latter gentleman succeeded completely, as far as his instrument allowed, in obtaining pictures of minute structures. Shortly after the publication of Mr. Talbot's process, various attempts were made to apply the oxyhydrogen microscope to the same purpose, as that instrument had superseded the solar. The result however was, that it was abandoned on account of the great time that was found to be necessary for impressing an image; and after a great variety of trials by Prof. Owen, Dr. Carpenter, Dr. Leeson and others, the use of the instrument for this purpose was given up.

The discovery of the collodion process, so much more sensitive than that of Mr. Talbot, led the author to think, as soon as he became acquainted with it, that we were in possession of the means of impressing microscopic objects by means of artificial light without any great trouble. A friend of his had an oxyhydrogen microscope of the common form, and on making a trial with it, he found that by using a very sensitive kind of collodion, he could obtain images by about a minute's exposure. On examining the instrument, however, he saw that its form must be completely changed, in fact, that an entirely new kind of instrument was required to obtain the best effect. The two points to be regarded as the peculiar principles of this microscope are, 1st, that none of the radiant light be lost, or as little as possible; 2ndly, that the magnifying power be obtained by such means as would not place the screen for receiving the image beyond such a distance from the object, that the motions of the

instrument could be governed at the same time that the image was closely inspected. The first of these objects is secured by giving a very large angular aperture to the system of lenses used for collecting the light, and by using another set of lenses for condensing it again on the object, and so arranging their focal length in proportion to the focal length of the object-glass, as to cover the plate to be acted upon, and that space only: the second, by using a sort of eye-piece for enlarging the image formed by the object-glass.

The lenses divide themselves into four groups, as represented in the figures, in which the light is supposed to proceed from the left hand to the right. The first set for collecting the light is composed of three large lenses, a meniscus, plano-convex and double convex, being a combination of three lenses similar in effect to Herschel's doublet; the second set for condensing the light on the object is a



similar set of lenses, but of much shorter focal length, and turned the other way; between these two sets is a plano-convex or plano-concave lens placed at its focal length from the convergence of the rays from the first group, so as to make the rays pass to the condensers in a state of parallelism, and so do away with the necessity of changing the distance between the collectors and condensers for each adjustment of the latter: the third group forms the object-glass, which must be so corrected as to have the rays of the spectrum between the fixed lines G and H as much as possible brought to a point, as these rays are those that produce the maximum action on the silver salts used in photography; this will require the red rays to be left untouched, just in the same way as Fraunhofer left those of the blue end of the spectrum dispersed in correcting an object-glass for light. The fourth group is the common eye-piece left under-corrected. A rather better form for this is a Ramsden's eye-piece with the first lens partially achromatized, by making it a compound lens with the radius of curvature of the common surface nearly double that of the surface that would render it achromatic.

This form of eye-piece gives a better correction of the oblique pencils than the common negative.

The time of exposure to obtain an intense negative six inches diameter, on a collodion plate prepared as below, is about a minute; a positive is obtained in a fraction of a second.

The collodion is formed by dissolving gun-cotton in sulphuric æther, and adding to it a small portion of iodide of silver dissolved in iodide of potassium, and also a very small portion of bromide of iron, or of iodide or bromide of arsenic. The image is developed by protonitrate of iron, or by a solution of pyrogallic acid in acetic acid and water, and fixed by a solution of hyposulphite of soda.

By taking out the two first lenses of the collectors, the instrument is adapted for using sunlight.

Note.—At the time that this communication was made to the Society, Prof. Stokes had kindly made known to the author the results of his discoveries with regard to the rendering visible the chemical spectrum, but as he had not then made them public, the author of this communication could not state the use that Prof. Stokes's discovery enabled him to make of a screen composed of uranium glass, or of infusion of horse-chestnut bark, for finding the focal distance of the chemical image, or of arranging the lenses of the condenser so as to produce the maximum of chemical action.

Also, since the communication was made, it has been found that the instrument described gives light enough to impress an image on any of the ordinary papers or Daguerreotype plates in periods ranging between one and five minutes, with the oxyhydrogen and lime light; and with direct sunshine the impression is almost instantaneous; of course sunlight is much better than any artificial light when it can be procured, both as regards speed and the clearness of the picture produced.

May 10, 1852.

Professor Miller gave an account of a new method of adjusting the Knife-edges of a Balance.

Also of a method of determining the height of clouds by night.

May 24, 1852.

Professor Stokes gave a Lecture on the Internal and Epipolic Dispersion of Light.

November 8, 1852.

Mr. Adams, F.R.S. &c., gave an account of some Trigonometrical Operations to ascertain the difference of geographical position be-

tween the Observatory of St. John's College and the Cambridge Observatory.

The observations, especially those of eclipses and occultations, which were made during many years by the late Mr. Catton at the Observatory of St. John's College, and which have recently been reduced under the superintendence of the Astronomer Royal, render it a matter of some importance to determine the exact geographical position of that Observatory. The simplest and most accurate means of doing this appeared to be, to connect it trigonometrically with the Cambridge Observatory. For this purpose, a base was measured along the ridge of the roof of King's College Chapel, by means of two deal rods terminated by brass studs, the exact lengths of which were determined by comparison with a standard belonging to Professor Miller. The extremities of the base were then connected by a triangle, with a station on the roof of the Observatory at St. John's, from which, as well as from the two former points, a signal post on the roof of the Cambridge Observatory could be seen. The angles at the extremities of the base, combined with the corresponding ones at the station at St. John's, furnished two determinations of the distance of the Cambridge Observatory, which served to check one another. The meridian line of the transit instrument at St. John's passes through King's College Chapel, so that by observing the point at which it intersected the base, the azimuths of the sides of the triangles could be immediately found.

The result thus obtained is, that the transit instrument of the Cambridge Observatory is 2313 feet to the north, and 4770 feet to the west of that at St. John's College. Hence it follows that the difference of latitude is $22''\cdot8$, and the difference of longitude $5''\cdot10$; and the latitude of the Cambridge Observatory being $52^\circ\ 12'\ 51''\cdot8$, and its longitude $23''\cdot54$ east of Greenwich, we have finally for the geographical coordinates of the Observatory of St. John's College,

Latitude.. $52^\circ\ 12'\ 29''\cdot0$
Longitude $0^\circ\ 0'\ 28''\cdot64$ E. of Greenwich.

These operations, of course, furnish incidentally, a very exact determination of the orientation of King's College Chapel. The line of the ridge of the roof points $6^\circ\ 20'\cdot3$ to the north of east.

November 22, 1852.

Professor Challis made a communication on the recent return of Biela's Double Comet.

December 6, 1852.

Professor Stokes gave an account of M. de Sénarmont's Researches relating to the Doubly-refracting Properties of Isomorphous Substances.

February 7, 1853.

An addition was read to a paper by Professor De Morgan on the Symbols of Logic, the Theory of Syllogism, &c.

A paper was read by Mr. Denison on some Recent Improvements in Clock Escapements.

The object of this paper was to explain the construction of a new *remontoire* or *gravity escapement* invented by the author, which has now been in action for some time on the pendulum of the great clock for the houses of parliament, and is in course of application to others, both turret clocks and astronomical.

But by way of introduction to this, which may be called the *three-legged gravity* escapement (from the form of the scape-wheel), Mr. Denison gave a description of another, which would similarly be called the *three-legged dead* escapement, and had been previously invented by him for the purpose of giving the impulse to the pendulum with far less friction than usual. He found that it required only $\frac{1}{5}$th of the force which a common dead escapement had required to make the pendulum swing the same arc. And therefore, as compared with a gravity escapement in which there is no sensible friction on the pendulum, there must be still more than $\frac{4}{5}$ths of the force in a common dead escapement wasted, in first producing friction on the pendulum, and then overcoming it by an increased impulse. The time of the pendulum would be much more disturbed than it is by the inevitable variations of this large amount of friction, as well as that of the clock train, but for a fortunate tendency of the different errors, which are caused by these variations of force and friction, to correct each other.

But the amount of this self-correction is uncertain, and sometimes one set of errors preponderates and sometimes the other; and so a dead escapement clock sometimes gains and sometimes loses simultaneously with either an increase or a decrease of the arc of vibration. And, consequently, none of the contrivances for isochronizing a pendulum for different arcs can secure isochronism of the clock; and no further material improvement in clocks can be expected, but from the solution of what has long been known as the great problem of clock-making, viz. the invention of a simple escapement which will give a constant impulse to the pendulum without any sensible friction.

Mr. Denison showed that his new gravity escapement satisfies all the requisite conditions, mechanical, mathematical, and œconomical. Its principal features are, that the scape-wheel has only three pins, not far from the centre, which lift the pallets or gravity-arms, and three long teeth which are locked by stops on the arms. The velocity of the scape-wheel, which usually produces *tripping*, if the force of the train is increased beyond what is just enough to lift the arms, is moderated by a fan-fly set on the axis of the scape-wheel. The arms are necessarily longer in this than in any other gravity escapement, and this also gives a greater depth of locking within a given angle, and therefore a still further security against tripping. And if an arm is by accident lifted a little too high, the tooth does not

escape, and the arm falls down again to its proper height until the pendulum carries it off, the pressure of the long teeth on the stops not being enough to hold it up. For these reasons also there is no difficulty in satisfying the mathematical condition investigated by Mr. Denison in a paper read before the Society in 1848, viz. that γ (the angle at which the pendulum leaves one arm and takes up the other) should $=\frac{\alpha}{\sqrt{2}}$, or at any rate not be less than $\frac{\alpha}{3}$ (α being the extreme arc of vibration). The escapement requires no oil in the parts affecting the pendulum; and it contains no delicate work, and is very easy to make; and as a highly finished train will be no longer necessary, astronomical clocks may be made on this plan much cheaper, as well as better, than heretofore.

In turret clocks an escapement of this kind supersedes the necessity for a remontoire in the train to equalize the force on the scape-wheel, and also of long and heavy pendulums, which are expensive when compensated, and are sometimes difficult to fix. It will also allow cast-iron wheels to be used throughout the clock (which Mr. Dent has now used for several years in connexion with Mr. Denison's spring remontoire for the train), as the friction of the train can no longer affect the pendulum.

February 21, 1853.

Professor Challis gave a Lecture on Halos, Parhelia, and Paraselenæ.

March 7, 1853.

Professor Stokes gave an account of some further researches relating to the Change of Refrangibility of Light.

April 11, 1853.

The Rev. Mr. Pritchard, F.R.S., gave an account of the Processes requisite to render Quicksilver tremorless for Astronomical Observation.

The great improvements recently introduced, and especially by the present Astronomer Royal, in the construction and methods of using astronomical instruments, require a far more extended use of reflexion from mercury than heretofore. Unfortunately, however, both the convenience and the accuracy of these methods have been greatly limited and impaired by the tremors to which mercury is liable. Many attempts have been made both in France and in Germany to remove or obviate these tremors, but hitherto by no means with perfect success. The Rev. C. Pritchard, of Clapham near London, has proposed a method which appears fully adequate to the requirements of astronomy. It consists in the adoption of a silver-plated or amalgamated copper vessel of a peculiar form, admitting the use of a very thin stratum of mercury without the necessity of an inconvenient amount of shallowness in the vessel itself. Mercury,

however, placed in an amalgamated vessel after a short time becomes covered with a singular film of amalgam, which impairs the reflecting power of the surface, and if at all agitated, soon entirely destroys it. And this is the case even when the vessel is made of amalgamated platina. The most important, and by far the most difficult part of Mr. Pritchard's experiments, consisted in the invention of a method by which these films can be easily and practically removed. The details, many of which are curious and interesting, would here occupy too much space, but they are fully explained in a memoir recently read to the Royal Astronomical Society of London; and it may be added, that the process has been adopted at the Royal Observatory at Greenwich, and is now in progress of trial at the Observatories of Paris and Cambridge.

April 25, 1853.

Professor Challis gave a lecture on the Adjustments of a Transit Instrument.

A paper was read by Professor De Morgan on the Principle of Mean Values, and an addition to a paper on the Symbols of Logic, &c. in vol. ix. part 1 of the Society's Transactions.

Though the heading of this paper describes one of its main results, yet it might with equal propriety have been styled a discussion of some points of algebra, with reference to the distinction of form and matter. This distinction, it is contended, is more extensively *applied* in algebra than in logic, though more *recognized* in logic than in algebra. Looking at the disputed points which exist in the higher parts of mathematics, and feeling satisfied that they will never be settled until the separation of form and matter is both visible and complete, the author makes a first attempt towards the examination of the question how far this yet remains to be done. A number of comparisons are made between algebraical and logical process, in the course of the inquiry, illustrative of the opinion entertained by Mr. De Morgan, that logic, as treated, requires the interposition of the algebraist, and cannot, except by aid of algebraical habits, be rendered a complete exposition of the forms of thought. In digressive notes, he combats the opinion that a generalization of the quantity is, as asserted, a new material introduction. He argues against the *too mathematical* tendency of some of the logicians who have endeavoured to extend the ancient system, especially the attempt of some to make the logical import of the proposition *nothing but* a comparison of more and less, and an equation or non-equation of quantities. He points out that the proposition has been formalised in nothing but its terms, subject and predicate; and gives an instance of the method in which a failure of general maxims is answered by the sole assertion that the mode of expression which brings about the failure is useless. He refers to what were called *sophisms*, contrasting the neglect of them by the logicians with the use which the algebraists have made of their corresponding difficulties, as in the case of negative and imaginary quantities, the fraction $\frac{0}{0}$, &c. He argues against the assertion of more than one eminent writer on logic, that the

identity of two terms, X and Y, expressed as in "all X is all Y," is *not* a complex proposition—is *not* the union of Every Y is X with Every X is Y. In an appendix to a former paper on the symbols of logic, he refers to a complaint of misrepresentation made by Sir W. Hamilton of Edinburgh, to whom certain technical phrases had been attributed. Mr. De Morgan makes the requisite correction, affirms that he had good reason for attributing such phraseology, and points out what that reason was: he then proceeds to answer two *new* charges of plagiarism against himself, from the same quarter; giving as his reason for addressing such answer to the Society, that Sir W. Hamilton makes the appearance of the asserted plagiarisms in the Transactions his principal ground of notice.

Finally, as to the logical part of the communication, Mr. De Morgan, reverting to his *complex* syllogism, in which each premise and the conclusion contain *two* ordinary propositions, generalizes the premises into the numerical form, and, giving terms and quantities algebraical designations, points out the mode of producing all possible inference. The immediate occasion of this introduction is as follows:—Sir W. Hamilton, in a recent publication, one tract of which is directed against Mr. De Morgan's last paper on syllogism, affirms that a proposition, as to its logical force, is *merely* an equation or non-equation of quantities, from which the declaration of coalescence or non-coalescence of terms into one notion is a *consequent*. Mr. De Morgan maintains the converse; namely, that the proposition is a declaration of coalescence or non-coalescence, of which the equation or non-equation of quantities is an essential. In treating the complex syllogism, under definitely numerical quantities, he has to search for the properties of the *equation of coalescence*, as distinguished from the *equation of quantity*; and, having made the former the means of arriving at inference, he invites those who can to try if the same result can be produced by means of the latter alone.

To pass to the algebraical part of the paper. It is first contended that the states *infinity* and *zero*, whether represented by distinctive symbols attached to 0 and ∞, or by negative and positive powers of dx, must be formally distinguished, as being each, not a value, but a *status*, containing an infinite number of *corrational* values, just as happens in finite quantity. In order to lay down the formal laws of connexion of these different states, it is necessary to examine the formal use of the symbol $=$. After pointing out instances in which the laws of algebra are by many declared invalid, as by those algebraists who admit and interpret $2x=x$, but cannot give permission to divide both sides by x, the following laws are suggested. The symbol $=$ is to be read with an index, as in $=_n$, which has reference to the order ∞_n or 0_{-n}, or as in $=_{-n}$, which has reference to ∞_{-n} or to 0_n. The equation $A=_n B$ is *normally* satisfied when A and B are of the order n, and $A-B$ of a lower order. It is *supernormally* satisfied if A and B be both of any (the same or different) higher order than the nth, and *subnormally* if both be of any lower order. Among the most conspicuous rules which follow, are that $AC=_{m+n}BD$ is normally satisfied, if $A=_m B$ and $C=_n D$ are so; and that when an

equation is multiplied or divided by a quantity of the order n, the index of equality must be increased or diminished by n. Various cases are given in which such results as now present anomalies are reduced under formal law, and others which would be absolutely rejected are shown to be capable of consistent interpretation.

The formal law of connexion of the different states, of which finitude (with the index 0) is only one, is that the order 0_m stands to finite quantity in all respects as finite quantity to ∞_m. Hence, so far as 1 and $1+0$ are *simultaneous* as well as equal, so far ∞ and $\infty+a$ are *simultaneous* as well as equal. And if $\varphi(1)={}_0\phi(1+0)$ be a universal law, so must be $\varphi(\infty)={}_0\varphi(\infty+a)$. Further, $\infty-\infty$ must be, formally speaking, wholly indeterminate, even when it is a case of $x-x$.

In relation to such indeterminate forms as $\infty-\infty$, $\frac{0}{0}$, &c., Mr. De Morgan contends that their formal and *à priori* character is that of indeterminateness; and that the choice between determinate and indeterminate character, which so often occurs, is dictated by the *matter of the problem*, the determinate value being dictated by the laws of algebra. The index of equality, for instance, may be the means of decision: an example is given in which one equation belongs to two different problems, but with different indices of equality; in one $\frac{0}{0}$ is determinate, in the other wholly indeterminate.

In assigning ∞ or 0 as *values*, it is often necessary to assign relations of order. When a quantity passes from positive to negative, or the converse, through 0 or ∞, it passes through every order of 0 or ∞; and this even when the passage is from one phase of 0 or ∞ to another, of different signs. Thus, the orders being powers, x cannot pass from $-a\,.\,0^m$ to $+a\,.\,0^m$, without passing through even 0∞.

Mr. De Morgan insists upon one of two things: either, the abandonment of the separate use of 0 and ∞, except only in the retention of the former symbol to represent $A-A$; or, the introduction of different orders, and the free use of the comparisons of those orders. For himself, he prefers and adopts the latter alternative.

The principle of limits is considered as a formal law of algebra, but not to the exclusion of every other result. If a constant, for instance, have the value A up to $x=a$ exclusive, it has A for one value when $x=a$. If the constant be *transitive*, that is, if it be always $=B$ after $x=a$, then $x=a$ gives *both* A and B for the constant, and, as a *fact* hitherto observed, its value from calculation is $\frac{1}{2}(A+B)$. This observed fact Mr. De Morgan believes he connects with the principle of limits, making it a necessary consequence of the universal truth of that principle; and hence he holds that it may be stated as a theorem, under the name of the *principle of mean values*. Various uses of this principle are given. Further, in assuming the free use of the orders of 0 and ∞, it is shown that it is correct to say that the constant passes from A to B while h, x being $a+h$, passes through the phases of 0. So that, for instance, at an epoch of transitiveness the value of $\phi(a+0)$ is dependent upon the form of 0. The brevity of an abstract prevents the statement of those cautions under which such use of language is introduced. One

result, however, may be brought forward. When the function fx is transitive (or, as commonly said, discontinuous) at $x=a$, the equation $\phi(fa)=(\phi f)a$ no longer necessarily exists. But this, as is pointed out, is what may happen at any value of x which makes a differential coefficient infinite.

On the question of $\sin\infty$ and $\cos\infty$, Mr. De Morgan deduces their *observed* values, $\sin\infty=0$ and $\cos\infty=0$, both from the principle of mean values, and from the formal truth of the equation $\phi(\infty+a)={}_0\phi\infty$. From the same principles follows the equation $(-1)^{\infty}=0$. In this case, however, and in all which come under the principle of mean values, the absolute necessity of the results is not affirmed. They are the *alternatives of indeterminateness*. But in thus representing them, Mr. De Morgan does not concede more than he conceives must be conceded with respect to $\infty-\infty$, $\frac{0}{0}$, and the like.

On the question of series, Mr. De Morgan contends that all the uncertainty and danger of divergent series belongs equally to convergent series, in every case in which the envelopment is unknown. On this part of the subject he adds to the arguments of a former paper, and insists upon the superior safety of the *alternating* series, in which the terms are alternately positive and negative.

Without going further into details, the purport of this paper may be stated as follows. Algebra, using the term in the widest sense, ought to be, and is approaching towards, a science of investigation, and a symbolic art of expression, of which the laws are strictly and without exception incapable of failure, suspension, or modification. The formal laws under which such a result is to be obtained, though laid down in the first instance by extensive induction, of which many steps are accompanied by difference of opinion, will at last be received and admitted as parts of the definition of the science, *à priori*. The existing defect of the science is an imperfect formalization, arising from the want of views of sufficient extent, and leading to *material* distinctions, that is, to exceptions dictated, *à posteriori*, by the results of particular cases. Such exceptions have in many instances been brought within rule by further consideration; and it is conceived that the same thing will happen at last in all cases. The paper is an attempt to examine the principal outstanding difficulties (those connected with the definition of integration excepted) with reference to the question how far they may arise from imperfect conception of formal laws. That there is to be a formal science, is positively assumed, and made the basis of the attempt: how far any suggestion contained in the paper is a valid step towards it, is treated with doubt and left to opinion.

May 9, 1853.

Mr. Hopkins, F.R.S. &c., the President of the Society, gave an account of some experiments for the determination of the temperature of fusion of different substances under great pressure; and on the application of the results to ascertain the state of the interior of the earth.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 14, 1853.

A paper was read by Mr. Dobson on the Theory of Cyclones. See Philosophical Magazine, vol. vi. p. 438.

Also, on the Storm-tracks of the South Pacific Ocean. See Philosophical Magazine, vol. vii. p. 268.

A communication was made by Mr. C. C. Babington on the use that has been made of the mode of growth to distinguish nearly allied Species.

November 28, 1853.

A paper was read by Mr. Wedgwood on the Geometry of the first three books of Euclid, synthetically demonstrated from premises consisting exclusively of definitions.

In a treatise* published by the author a few years ago, definitions founded on relations of direction were indicated as exhibiting the ultimate analysis of the conceptions of straightness and parallelism in lines, and of planeness in surface; and in proof of the adequacy of these definitions as the basis of a complete system of geometry without the aid of axioms or any other assumption whatever, they were employed in demonstrating the principal propositions necessary to place the student on the ground occupied by the definitions and axioms of the ordinary system. If the basis thus built in underneath the old foundations of the science had been complete in every nook and corner, nothing more would have been required in order to rest the entire demonstration on the single principle of definitions. So long, however, as any step in the process, however subordinate, was left to be supplied by others, there always would be room for suspicion that the assumption in reasoning which was speciously plastered over in one place might be secretly undermining the system in another. The reform, moreover, of the premises in geometry is a problem on which such an infinity of thought has been spent, and

* The Principles of Geometrical Demonstration deduced from the original conception of Space and Form. Taylor and Walton. 1844.

to which so many answers, more or less plausible, have been offered, that nothing short of a complete exposition of a consistent scheme of demonstration can be expected to carry conviction in the validity of a fresh solution. The object of the present paper is accordingly to complete the task undertaken in the foregoing publication by a formal statement of the other definitions required in connexion with those of straight and parallel lines and plane surface, and by a rigid demonstration from these premises of the steps intervening between those and the premises of the ordinary system; and in additional proof of the fundamental character of the proposed analysis, the demonstration is carried on through the geometry of the three first books of Euclid by direct reasoning, without resort to the comparatively unsatisfactory method of *ex absurdo* proof, which, although equally conclusive as to the necessity of the result, yet always leaves a hankering in the mind for an answer why the case must be as the demonstration shows that it cannot avoid being.

In the execution of the foregoing plan, the whole of the problems of Euclid are omitted as irrelevant to the demonstration of the other propositions. The grounds on which they were adopted in the system of Euclid appear to be these. It frequently happens that it is necessary in the course of demonstration to make some new construction not included in the figure which forms the original subject of the proposition, and it was evidently thought that the geometer would not in strictness be entitled to take such a step until he had demonstrated the means of executing it with exactitude. The student was accordingly in the postulates put in possession of a ruler and a pair of compasses; and wherever any additional construction was required in the proof of a proposition, a problem was premised, showing the means by which the construction might be made by the aid of those implements.

But it should be recollected that the figure by which the demonstration is commonly accompanied is not the actual subject of the reasoning, but a mere illustration to aid the imagination and the memory, the exactitude of which is matter of comparative indifference. Moreover, the principle on which the problems are introduced is not consistently carried out to its legitimate conclusion even in Euclid. There is no difference in the reasoning between the figure which forms the original subject of the proposition, and the additional construction which is made in the course of demonstration; and therefore if it were necessary for the validity of the conclusion to demonstrate the means of executing the latter figure, it would be equally necessary in the case of the former. The student would not be entitled to move a step in the demonstration of the equality of two triangles having two sides and the included angle equal, until he had been taught how to construct two such triangles, and consequently how to describe an angle equal to a given angle. The demonstration in Euclid begins with perfect legitimacy. "Let ABC, DEF be two triangles in such and such conditions," without the necessity of indicating the means by which those conditions may be mechanically executed, or indeed of their possibility of actual exist-

ence; and it may with equal legitimacy proceed to exemplify in like manner any further construction which may be found necessary in the course of demonstration.

The question of motion has commonly been considered so essentially distinct from that of position, that all reference to the former subject has rigorously been excluded from the field of geometrical inquiry. But the position of every point must ultimately be determined by motion from points antecedently known, and to the incidents of motion we should accordingly look for the original source of the relations of position. Now motion (in as far as it influences position) admits of variation in two ways; viz. in the direction of the motion at each indivisible instant of time, and in the length of the track accomplished in a finite period; whence it has been said by Sir John Herschel that space (which is primarily known as the receptacle of motion) is reducible in ultimate analysis to distance and direction.

The relations of extent are simply those of equal, greater, and less, with respect to which it will be necessary only to define the test by which they are respectively to be demonstrated in concrete figure. The relations of direction are of a much more complicated nature. The different phases of this elementary attribute of motion are distinguished, not, like those of colour, by a permanent character independently cognizable in each individual, but more like musical notes, by their relative position on a peculiar scale which may be made to rest on any individual as an arbitrary basis.

The scale by which directions are compared is founded on the elementary relations of opposition and transverseness. In whatever direction we suppose ourselves to be traversing space, we recognize the possibility of returning to the same position from whence we set out by motion in a different direction, the relation of which to the original is that of opposition; or the two may be classed together as the positive and negative modifications of a common direction.

Again, if we fix our thoughts upon any given direction, we find a series of others in each of which it is possible to traverse space without advance in the original direction or in the one opposed to it. The directions so marked out by negation of progress in a certain direction are said to be *transverse* to the *normal* or direction in which no progress is made by the observer while advancing in the direction of any of the transverse series. If now we start afresh from any of the individuals of the latter series, it will be found that the series includes the opposite direction, as well as one direction and its opposite transverse to the former two. Every other individual of the series will be recognized as partaking in different proportions of the nature of these coordinates, or transverse directions, adopted as the basis of the scale. In other words, it will be found that distance in any intermediate direction is essentially composed of distance in the direction of each of the coordinates in different proportions, varying from all of the one and none of the other, to all of the latter and none of the former, with every modification arising from taking each of the coordinates in both a positive and a negative sense.

In like manner, as each intermediate direction is transverse to the original normal, a secondary series of directions with a different normal will arise from the combination of these coordinates in every proportion, and the whole expanse of space around the observer will be recognized as consisting of distance in every possible combination of proportions in the direction of three coordinates, of which the first may be taken at pleasure in space, the second may be identified with any of the series transverse to the first coordinate, and the third will be the single direction transverse to each of the former two. Within the sphere of three directions so related to each other we are entirely shut in. Whatever may be the particular direction in which the coordinates be laid, we can conceive no fourth direction essentially differing in nature from the former three, and therefore can conceive no possible direction which cannot be derived from some combination of three coordinates, or in which a given distance cannot be resolved into equivalent distances in the direction of the three coordinates.

We have thus in the relations of transverseness and opposition, and in the conception of intermediate directions arising from the combination of transverse coordinates in different proportions, a uniform scale by which, when applied to known directions in space, the position of any other direction may be accurately defined independent (it must be observed) of any reference to the notion of angular magnitude, of which as yet no mention has been made.

When two directions only are known in a system, they must be considered as members of the series transverse to a common normal; and one of the two being identified with the first coordinate of the scale, the position of the second will be completely determined by the proportion in which it partakes of the nature of the second coordinate or transverse direction of the series.

The directions commonly adopted as the basis of the scale, are the up and down, fore and aft, and right and left lines marked out (in any given position of the observer in a system) by the constitution of his bodily frame; and thus (in any given position of our bodies) a particular direction is defined in our thoughts by the proportion in which it partakes of the nature of those coordinates, that is to say, by the proportion in which distance in the direction in question is essentially composed of distance up or down, distance to the front or rear, and of distance to the right or left.

For the sake of simplifying the question, we will now confine our thoughts to motion in a plane surface, or to directions having reference to two transverse coordinates. Now although, in the actual apprehension of a figured system, the observer must be supposed to traverse the entire outline, and thus continually to change his place, yet he must be capable of doing so without rotation on his own axis, as he would otherwise acquire no notion of the configuration of his track in the external system. He will accordingly carry with him throughout the fundamental conceptions of front and back, right and left, and by reference to these coordinates will be able to compare and to identify directions in any part of the system.

It is in virtue of this complex scheme of relation between directions, that we are enabled to conceive the possibility of reaching the same point by different tracks from a common starting-point. We are indeed so much in the habit of thinking of points as marked out by physical phænomena (as by the letters in a geometrical illustration), that it is by no means obvious where the difficulty of the conception lies. But it must be remembered that points in geometry are distinguished solely by position, while the position of a given point is determined by the nature of the track by which it is reached from a point antecedently known. It is plain, therefore, that there would be no means of identifying points attained by tracks differing in any respect from each other, if the precise combination of distance and direction by which they were respectively attained were the ultimate test of their position. But now the knowledge of the fundamental scheme of relationship above explained makes us regard the space traversed in each successive instant of time in the track by which the position of a point is determined (and consequently the whole space traversed in the entire track), as equivalent to a certain distance in the direction of each of the two coordinates of the scale. The aggregate character (in respect of distance and direction) of the space traversed in different tracks (by which the position of the terminal points is governed) will thus be made to depend on the aggregate distance advanced in the direction of the two coordinates, a question to be tried by simple superposition. When the distance advanced in the direction of each coordinate is the same, the positions finally attained will be recognized as identical, and the points will coincide whatever may be the amount of intermediate divergence in the tracks by which they have actually been reached.

From the same principle it may be shown, that a straight line may be drawn from a given point to any other point in space. Because the space traversed in the track by which the second point must be supposed to have been determined, will be equivalent in distance and direction to a certain distance in each of the two standard directions of the system. Now inasmuch as the series of directions intermediate between any pair of transverse directions includes individuals partaking in every conceivable proportion of the nature of both the transverse directions between which they lie, it will always be possible to select one of the series a certain distance in which will be equivalent to given distances in each of the two transverse directions, and therefore the distances in the direction of the coordinates of the system under consideration, into which the space traversed in the original track has been resolved, may again be exchanged for an equivalent distance in a single direction duly related to each of the coordinates; in other words, the same position may be attained by motion in a single continuous direction as by a track of any other description, or what amounts to the same thing, a straight line may be drawn from a given point to a point determined by a track of any other description.

As soon as a straight line is known as lying in a single continuous

direction, it becomes the most obvious means of marking the direction so exhibited throughout a finite extent of line. The series of directions transverse to a given normal may then be represented by two straight lines crossing each other at right angles, and an indefinite number of other straight lines diverging from the point of intersection, and dividing the plane surface round that point into as many parts as there are diverging lines. If now we take two of these lines, like the hands of a clock, and suppose one to remain fixed while the other revolves from left to right, it will pass successively through all the directions intermediate between left and front, while the quantity of plane surface intercepted between the hands abutting on the point of intersection will continually increase as the difference in their direction becomes greater, or in proportion as distance in the direction of the moveable hand contains a greater proportion of distance in the direction transverse to that of the fixed one. Thus we are taught a new mode of estimating the relation between the direction of straight lines diverging from a common point; not by a proportion which addresses itself to the understanding merely, but by a quantity admitting of measurement by bodily comparison, viz. by the quantity of plane surface intercepted between the diverging lines and abutting on the point of intersection, or by the magnitude of the included angle.

Professor Challis gave an account of a luminous appearance observed at the time of the perihelion passage of Klinkerfue's comet.

Professor Stokes read a paper on the Optical properties of Light reflected from Crystals of Permanganate of Potash. The substance of this paper is embodied in a paper on the Metallic Reflexion exhibited by certain Non-metallic Bodies, published in the Philosophical Magazine, vol. vi. p. 393.

December 12, 1853.

Professor Fisher read the first part of a paper, entitled "Researches, Physiological and Pathological, on the Development of the Vertebral System."

After having explained what he meant by the term *vertebral system*, he stated (and he illustrated what he described by drawings) that the spinal marrow, at a particular stage of growth of the human embryon, exhibits indications of segmental development corresponding to that of the spinal column; that is to say, that each of its halves offers on its external surface a series of symmetrical spaces defined by transverse lines, each of which spaces corresponds to the roots of a single spinal nerve; and again, that each half presents in its internal structure, a double series, one anterior, the other posterior, of symmetrical areas, two of which appeared to equal in extent one of the external spaces just spoken of. Professor Fisher also

stated that the spinal marrow offers, at the period of development in question, several other peculiarities, some of them bearing likewise a segmental character; but he reserved a detailed description of them for a future communication.

February 27, 1854.

A paper was read by Professor Challis, entitled "A direct Method of obtaining by Analysis the mean motions of the apse and node of the Moon's Orbit." See Philosophical Magazine, vol. vii. p. 278.

Also a paper by Mr. J. B. Phear on some parts of the Geology of Suffolk, particularly with reference to the Valley of the Gipping.

The deposits which constitute what is often termed the glacial formation, but which the present state of our knowledge hardly allows us to designate by a name significant of a common origin, present so much confusion to the inquirer, and impose upon him so much laborious research by the extent and the unconnected character of their distribution, that they have hitherto met with less attention than their importance deserves.

The county of Suffolk seems to be a district where a portion of these deposits is manifested with more than usual distinctness, and is capable of being studied with comparative facility. The county is separated from Norfolk on the north by the well-marked valleys of the Ouse and the Waveney, is bounded on the east and south by the sea and valley of the Stour, and is bordered by chalk uplands on the north-west; the whole central portion is thickly covered with a mass of blue drift-clay, cut into abrupt undulations by a network of valleys. This clay is totally without any symptom of stratification, and is full of fragments of all rocks of the secondary period, including specimens of granite and other igneous rocks.

Wells sunk in different parts of the county show this drift-clay to have a thickness varying from 200 feet to a few inches; it seems to thin off from the northern and western parts of the county towards the coast, and only exists in the shape of outliers beyond a line passing through Sudbury, Hadleigh, Bramford, Woodbridge, and Saxmundham; a line, it may be remarked, nearly coinciding with the edge both of the London clay and of the crag, and approximately passing through the heads of the tidal estuaries of the Orwell, Deben, Ore and Alde. The clay is almost universally underlaid by an unfossiliferous sand; and there is reason to conjecture that this sand, of a prevailing red colour, passes out beyond the just-mentioned line, and covers in many places the surface of the strip of land between it and the sea.

A detailed examination of the Gipping valley reveals a well-marked and connected line of sand cliffs fringing it, and its Coddenham tributary in particular, at a high level on both sides; the sand is generally pure white, though often red, horizontally stratified and

capped with an unrolled gravel, which evidently owed its existence to the quiet washing away of the drift-clay from its insoluble contents. Above Needham Market the valley is channelled in drift-clay, but between Needham and Bramford it is cut through chalk; and it should be remarked, that the line of sand-hills does not extend up the valley with any great distinctness beyond the chalk. The phænomena seen at Creeting are not consistent with this sand lying beneath the drift-clay; and the inference is, that it constitutes the remains of an estuary deposit formed in the valley subsequent to its excavation in the drift-clay.

All the other streams west of the Gipping have chalk for their floor during the middle part of their course, thus manifesting the existence of a ridge of chalk running beneath the drift accumulations nearly due west and east from Sudbury to Bramford. Disturbances evidenced in this ridge, and perhaps due to its elevation, are partaken of by the London clay and crag deposits which overlie it on the east and south.

In Norfolk the drift-clay attains a greater thickness than in Suffolk, and towards the north of the county is overlaid by a sand and gravel formation which may be appropriately termed upper drift. The gradual disappearance of this towards the south, together with the thinning away and final extinction of the drift-clay in the same direction, point to a region of greater denuding activity; it may be an interesting question whether such denudation be in any degree connected with the upheaval of the before-mentioned chalk ridge, or again, whether the sands of the Gipping valley bear any relation to the upper drift of Norfolk.

March 13, 1854.

A paper was read by Prof. Challis on the Eccentricity of the Moon's Orbit; supplement to a former communication on the mean motions of the Apse and Node. See the former paper, Phil. Mag. vol. vii. p. 278.

Also a paper by Mr. J. Clerk Maxwell on the Transformation of Surfaces by Bending.

The kind of transformation here considered is that in which a surface changes its form without extension or contraction of any of its parts. Such a process may be called bending or development. The most obvious case is that in which the surface is originally a plane, and becomes, by bending, one of the class called "developable surfaces." Surfaces generated by straight lines, which do not ultimately intersect, may also be bent about these straight lines as axes. In this way they may be transformed into surfaces whose generating lines are parallel to a given plane, just as the former class are transformed into planes.

In both these cases, the bending round one straight line of the

system is quite independent of that round any other; but in those which follow, the bending at one point influences that at every other point. The case of a surface of revolution bent symmetrically with respect to the axis is taken as an example.

The remainder of the paper contains an elementary investigation of the conditions of bending of a surface of any form.

The surface is considered as the limit of the inscribed polyhedron when the number of the sides is increased and their size diminished indefinitely.

A method is then given by which a polyhedron with triangular facets may be inscribed in any surface; and it is shown, that when a certain condition is fulfilled, the triangles unite in pairs so as to form a polyhedron with quadrilateral facets. The edges of this polyhedron form two intersecting systems of polygons, which become in the limit curves of double curvature; and when the condition referred to is satisfied, the two systems of curves are said to be "conjugate" to one another.

The solid angle formed by four facets which meet in a point is then considered, and in this way a "measure of curvature" of the surface at that point is obtained.

It is then shown that if there be two surfaces, one of which has been developed from the other, one, and only one, pair of systems of corresponding lines can be drawn on the two surfaces so as to be conjugate to each other on both surfaces. This pair of systems completely determines the nature of the transformation, and is called a double system of "lines of bending." By means of these lines the most general cases are reduced to that of the quadrilateral polyhedron. The condition to be fulfilled at every point of the surface during bending is deduced from the consideration of one solid angle of the polyhedron. It is found that the product of the principal radii of curvature is constant.

By considering the angles of the four edges which meet in a point, we obtain certain conditions, which must be satisfied by the lines of bending in order that any bending may be possible. If one of these conditions be satisfied, an infinitesimal amount of bending may take place, after which the system of lines must be altered that the bending may continue. Such lines of bending are in continual motion over the surface during bending, and may be called "instantaneous lines of bending." When a second condition is satisfied, a finite amount of bending may take place about the same system of lines. Such a system may be called a "permanent system of lines of bending."

Every conception required by the problem is thus rendered perfectly definite and intelligible, and the difficulties of further investigation are entirely analytical. No attempt has been made to overcome these, as the elementary considerations previously employed would soon become too complicated to be of any use.

For the analytical treatment of the subject the reader is referred to the following memoirs:—

1. "Disquisitiones generales circa superficies curvas," by M. C.

F. Gauss (1827).—*Comm. Recentiores Gott.* vol. vi.; and in Monge's "Application de l'Analyse à la Géométrie," edit. 1850.

2. "Sur un Théorème de M. Gauss, &c.," par J. Liouville.—*Liouville's Journal*, 1847.

3. "Démonstration d'un Théorème de M. Gauss," par M. J. Bertrand.—*Liouville's Journal*, 1848.

4. "Démonstration d'un Théorème," Note de M. Diguet.—*Liouville's Journal*, 1848.

5. "Sur le même Théorème," par M. Puiseux.—*Liouville's Journal*, 1848.

And two notes appended by M. Liouville to his edition of Monge.

March 28, 1854.

Prof. Miller gave an account of the relation between the physical characters and form of crystals of the oblique system as established by the observations of Mitscherlich, Neumann, De Senarmont, Wiedemann and Ångström.

A paper was read by Prof. De Morgan on some Points in the theory of differential equations.

1. The words *primordinal*, *biordinal*, &c. are used in abbreviation of the phrases 'of the first order,' 'of the second order,' &c.

The symbol for a differential coefficient, U_x for $\frac{dU}{dx}$, &c., long used by the author, is thus extended. By $U_{x|p,q}$ is meant $dU : dx$ with reference to x as contained in p and q, as well as explicitly. Thus $U_{x|p,q}$ means $U_x + U_p p_x + U_q q_x$; and $U_{x|y}$ means $U_x + U_y y'$.

Differentiations are sometimes expressed thus: $d_x U = U_x dx$, $d_{x,y} U = U_x dx + U_y dy$.

When it is only requisite to express functional relation, without specification of form, $(x, y, z) = 0$ or $z = (x, y)$ may signify an equation between x, y, and z. A letter may be used as its own functional symbol: thus $u = u(x, y, z)$ may signify that u is a function of x, y, z. And in 'for u write $u(x, y, z)$' there is a convenient abbreviation of 'for u substitute its value in terms of x, y, z.'

2. When, as so often happens, a variable enters under relations which destroy the effect of its variation upon the *form* of differential coefficients, it is called *self-compensating*. Thus $\phi(x, y, a) = 0$, $\phi_a(x, y, a) = 0$, contain the self-compensating variable a. Similarly, when $\phi(x, y, a, b) = 0$ is accompanied by $\phi_a da + \phi_b db = 0$, a and b are mutually compensative, and primordinally. The addition of

$$\phi_{a(x|y)}\, da + \phi_{b(x|y)}\, db = 0$$

makes a and b biordinally compensative.

3. When a finite change in x makes an infinite change in y, it makes an infinite change in $y' : y$, in $y'' : y'$, &c. When *either* or

both P and Q become infinite, $P : Q$ and $P_x : Q_x$ are both nothing, both finite and equal, or both infinite; provided that the infinite form is produced by *substitution for x*. If $u=(v, w, \ldots)$, any relation which makes u_v infinite either makes u_w infinite, or is independent of w. And if $u_v=\infty$ be produced by a relation containing v, then $u_v dv+u_w dw+\ldots=0$ and $u_{vw}dv+u_{vw}dv+\ldots=0$ are relations of identical meaning.

4. From the last it follows that $U=$const. is solved by making any factor of dU either 0 or ∞. In $dU=M(Pdx+Qdy)$, singular solutions are obtained, as is known, from $M=\infty$: it ought to be asked whether $M=0$ does not give singular *exceptions*, that is, cases in which $U=$const. arises otherwise than from $P+Qy'=0$. It is found more convenient to treat these cases without actual separation of the factor; that is, from $dU=U_x dx+U_y dy$.

5. In a former paper, the author insisted on the arbitrary functions which enter the intermediate primitives: maintaining, for example, that the primordinal of $y''=0$ is $\varphi(y', xy'-y)=0$, for any form of φ. Lagrange, he has since found, notices this extension, and rejects it, because it leads to $y'=a$, $xy'-y=b$, as necessary consequences of its ordinary solution. Mr. De Morgan maintains his opinion, and observes that Lagrange's reason would make it imperative to reject one of the two, $y'=a$, $xy'-y=b$, since either is the necessary consequence of the other.

6. In order to avoid the ambiguous use of the word *singular*, a singular solution is defined as any one which, by the mode of obtaining it, cannot have the ordinal number of constants: it is further styled *intraneous* or *extraneous*, according as it is or is not a case of the general solution. If $y=\psi(x, a)$ or $a=A(x, y)$ give $y'=\chi(x, y)$, then $dA=A_y(y'-\chi)dx$ and $\chi=-A_x : A_y$ are identical equations. Every relation which satisfies $A_y=\infty$ *is* a solution, and a singular solution; except possibly, relations of the form $x=$ const., which must always be examined apart. Also, $A_y=\infty$ is identical with $\psi_a=0$. There can exist no solutions whatsoever except those which are contained in $A=$const., $A_y=\infty$, and (possibly) $x=$const.

Again, $\chi_y=(\log\psi_a)_x$. Of this equation the author has found neither notice nor use: supposing it to have ever been given, he holds it most remarkable that it has not become common as the mode of connecting the two well-known and widely used tests of singular solution. It easily shows that $\chi_y=\infty$ contains all extraneous solutions, and all intraneous solutions which (as often happens) can be also obtained by making a a function of x. It also easily gives a conclusion arrived at by the author in his last paper, namely, that when $\chi_y=\infty$ is satisfied and not $y'=\chi$, it follows that $\chi_x+\chi_y\chi$ is infinite.

7. The author gives his own version of the demonstration of a theorem of M. Cauchy, for distinguishing extraneous and intraneous solutions. If $y=P$, P being a given function of x, satisfy $y'=\chi(x, y)$, that is, if P' and $\chi(x, P)$ be identical, then $y=P$ is an extraneous or

intraneous solution of $y'=\chi(x, y)$, according as

$$\int_{P}^{P+\beta} \frac{dy}{\chi(x,y)-\chi(x,P)}$$

(x being constant) is finite or infinite for small values of β. This theorem has attracted little notice in this country: the author believes it to be fully demonstrated, and considers it one of the most remarkable accessions of this century to the theory of differential equations.

8. It is observed that the validity of the extraneous solution may depend upon the interpretation of the sign of equality by which $A=B$ is held satisfied when both sides are 0, or both infinite, even though $A:B=1$ is not satisfied. Thus $y'=2\sqrt{y}$ or $y=(x+a)^2$, has the extraneous solution $y=0$, which, however, is not a solution if by $y'=2\sqrt{y}$ we understand in *all* cases $y':\sqrt{y}=2$.

9. The common mode of obtaining the singular solution of a biordinal (by combining $\phi(x, y, a, b)=0$, $d_{a,\,b}\phi=0$, $d_{a,\,b}\phi_{x\,|\,y}=0$) though sufficiently general, is never *shown* to be so.

Let $y=\psi(x, a, b)$, combined with $y'=\psi_x$, give $a=A(x, y, y')$, $b=B(x, y, y')$, from either of which follows $y''=\chi(x, y, y')$. The most general primordinal is $f(A, B)=0$, f being arbitrary. Any given curve, $y=\varpi x$, may be made to solve this for some form of f; but, generally speaking, this solution will be *extraneous*. For A and B are so related that every intraneous solution makes A and B constant. And any primordinal equation whatever may in an infinite number of ways be thrown into the form $f(A, B)=0$, so that the intraneous solutions shall make A and B constant.

(Given $y=\varpi x$, required a key to all the primordinals of which it is a singular solution. Take *any* equation $y=\psi(x, a, b)$, eliminate x between $a=A(x, \varpi x, \varpi' x)$ and $b=B(x, \varpi x, \varpi' x)$, and write $A(x, y, y')$ and $B(x, y, y')$ for a and b in the result.)

The equations $dA=A_{y'}(y''-\chi)dx$, $dB=B_{y'}(y''-\chi)dx$ are identically true. And $A_{y'}=\infty$, or any relation which satisfies it, is a singular primordinal of $y''=\chi$, whenever it is a primordinal at all; that is, when y' appears in it. When $A_{y'}=\infty$ is satisfied by a relation void of y', that relation is not necessarily a solution. The ordinary solutions of $A_{y'}=\infty$ are solutions of $y''=\chi$; but not (necessarily) the singular solutions. The singular solutions of a relation which makes $A_{y'}=\infty$ may make $A_{y'}$ finite.

Comparing A and B with ψ, we have

$$A_{y'}=-\frac{\psi_b}{\psi_a\psi_{bx}-\psi_b\psi_{ax}}, \quad B_{y'}=\frac{\psi_a}{\psi_a\psi_{bx}-\psi_b\psi_{ax}},$$

$$\chi_{y'}=\{\log(\psi_a\psi_{bx}-\psi_b\psi_{ax})\}_x.$$

From these are obtained results in complete analogy with those for primordinal equations. But when $\psi_a\psi_{bx}-\psi_b\psi_{ax}=0$, the usual criterion of singular solution, is made valid by $\psi_a=0$, $\psi_b=0$, a singular primitive of the singular primordinal may be obtained, which does not necessarily satisfy $y''=\chi$.

10. Similar forms are given for *triordinal* equations. In noticing the manner in which the equations of the general theory may be easily expressed by what are called *determinants*, Mr. De Morgan expresses his admiration of the system, and his sense of the important services rendered by those who have laid its foundations. But he refuses to employ the word *determinant* in the sense proposed, on account of its not expressing any *distinctive* property of these functions. Until those who have a better right to give a name provide themselves with a *distinctive* one, he intends to call them *eliminants*.

The forms connected with $y=\psi(x, a, b)$ may be easily translated into others derived from $\phi(x, y, a, b)=0$. But the formula which connects $\chi_{y'}$ with ϕ is as follows:—

$$\chi_{y'}=\left\{\log\left(\frac{\phi_a\phi_{bx'y}-\phi_b\phi_{ax'y}}{\phi_y^2}\right)\right\}_{x'y}+\frac{\phi_a\phi_{by}-\phi_b\phi_{ay}}{\phi_a\phi_{bx'y}-\phi_b\phi_{ax'y}}\cdot\chi,$$

where by $U_{x|y}$ is meant $U_x+U_y y'$, even when U is a function of y'. Thus $(xy'-y)_{x|y}$, as here used, is 0.

11. The following idea of reciprocal polarity has been presented by M. Druckenmüller (as cited from Crelle's Journal by Mr. Boole), and, independently, by Professor Boole: it occurred to the author of this paper before he had seen the researches of either. If there be equations involving $m+n$ variables, and if, determining a point by fixing m of the variables, a curve be determined by giving all possible values to the remaining n (*point* and *curve* being here merely names of objects determined), we may say that the (m)-point is the pole of the (n)-curve. Similarly, we may make each (n)-point the pole of an (m)-curve. And all the points of any curve have polar curves which contain the pole of that curve. If the two sets of variables be severally made primordinally compensative, the general properties which arise are easy extensions of the well-known theory of reciprocal polars. Let (x, y) and (a, b) be two points: the polar property of $x^2+y^2=ax+by$ contains the direct and converse property of the angle in a semicircle. If $\phi(x, y, a, b)$ be the *modular equation*, and if x, y and a, b be compensative, any element (x, y, y') of *any* (x, y)-curve to the pole (a, b) determines an element (a, b, b') of an (a, b)-curve to the pole (x, y). These curves are *reciprocal polars*. In the common system, the modular equation is linear with respect to both pairs of coordinates, and the locus of those poles which lie in their polar straight lines is a conic section, to which the polars are tangents.

12. The method of transforming differential equations, given by the author in his last paper, is precisely the reference of the curves sought to their reciprocal polars, the modular equation being taken at pleasure. Mr. De Morgan now proposes to call it the method of *polar transformation*. Let $\phi(x, y, a, b)=0$ be the modular equation, and let $\phi_x+\phi_y y'=0$, $\phi_a+\phi_b b'=0$, b' being $db:da$. Hence

$$a=A(x, y, y'),\ b=B(x, y, y');\quad x=X(a, b, b'),\ y=Y(a, b, b')$$
$$b'=B_{y'}\div A_{y'}\qquad\qquad y'=Y_{b'}\div X_{b'};$$

the biordinal factors, $y''-\chi(x, y, y')$, $b''-\alpha(a, b, b')$, disappearing

from b' and y'. Hence b' depends on x, y, y'. Similarly, b'' depends on x, y, y', y'', &c., and similarly for y', y'', &c. If in $f(x, y, y', y'', \&c.)=0$ we substitute for x, y, y', &c. in terms of a, b, b', &c., the two equations belong to polar reciprocals. If either can be integrated, the integration of the other depends on elimination: thus if the equation in a, b, &c. can be integrated, the solution of the equation in x, y is obtained by eliminating a and b between the integral obtained and $x=\mathrm{X}$, $y=\mathrm{Y}$.

13. There are two reciprocal biordinal equations belonging to the modular equation $\phi(x, y, a, b)=0$; $y''=\chi$ when a and b are constant, $b''=\alpha$ when x and y are constant. The two have the same condition of singular solution; for $\mathrm{A}_{y'}\phi_b=\mathrm{X}_{b'}\phi_y$. Let this be $\sigma(x, y, a, b)=0$, when cleared of y' or b'. The following table exhibits the relations of the double system:—

$$\phi(x,y,a,b)=0$$

$$\sigma(x,y,a,b)=0 \qquad \phi_x+\phi_y y'=0 \qquad \phi_a+\phi_b b'=0 \qquad \sigma(x,y,a,b)=0$$

$$y'=\varpi(x,y) \quad a=\mathrm{A}(x,y,y') \quad b=\mathrm{B}(x,y,y') \qquad x=\mathrm{X}(a,b,b') \quad y=\mathrm{Y}(a,b,b') \quad b'=\lambda(a,b)$$

$$y=\Pi(x, \mathrm{C}) \qquad y''=\chi(x,y,y') \qquad b''=\alpha(a,b,b') \qquad b=\Lambda(a,\mathrm{Z}).$$

Eliminate a and b between $\phi=0$, $\sigma=0$, $\phi_{x|y}=0$, and we have $y'=\varpi$, $y=\Pi$, the singular primordinal and primitive of $y''=\chi$; those of $b''=\alpha$ are obtained by eliminating x and y from $\phi=0$, $\sigma=0$, $\phi_{a|y}=0$. There is a relation involved between C and Z, the constants of integration. For each value of C, $y=\Pi$ is the xy-curve which touches all in $\phi(x, y, a, \Lambda)=0$, for the corresponding value of Z and all values of a. The same of Z, $b=\Lambda$, and $\phi(x, \Pi, a, b)=0$. The contacts are of the second order, and $y=\Pi$, $b=\Lambda$, are polar reciprocals for corresponding values of C and Z. But the singular primitives of $y'=\varpi$ and $b'=\lambda$ are not necessarily reciprocals: when this does happen, their contacts with primitives are of the third order.

14. When a surface is described by one set of curves, as in the surface obtained by eliminating a from $\phi(x,y,z,a)=0$, $\psi(x,y,z,a)=0$, it is proposed to call it a *shaded* surface, and the curves *lines of shading*. The equation $f(x, y, z, y', z')=0$, y and z being functions of x, cannot, generally, belong to any family of surfaces in an unrestricted sense; that is, it cannot be always true of a point moving in any way upon a surface. Such a supposition would be equivalent to imagining a surface *every* point of which has the primordinal character of the vertex of a cone. But it may belong to any surface, properly shaded, or to any mode of shading, if the proper surface be chosen.

15. Two equations of the form $y=\Phi(x, a, b, c)$ $z=\Psi(x, a, b, c)$, give one, and only one, primordinal of the form $f(x, y, z, y', z')=0$. Assume any surface $\omega(x, y, z)=0$; by this, and compensative relations between a, b, c, another pair of primitives may be found. But the primitives obtained from $\omega=0$ do not *shade* this surface, except in cases determined by two relations between the constants. Again, making a, b, c compensative, without any assumed surface, we find

one equation of the form $(a, b, c, a', b')=0$, any primitives of which lead to other primitive forms for $f=0$. Each of the second primitives has contact of the first order with one family of curves from among the original primitives; and all ordinary primitives are found, in an infinite number of ways, among the connecting curves of others. There is a singular solution, a curve of contact to all primitives, when $\Phi_a=0$, $\Psi_a=0$, &c. can all be satisfied at once.

Since $y=\Phi$, $z=\Psi$, give a primordinal equation independent of constants, the polar reciprocal properties of curves in space are of a restricted form. Every surface dictates another surface, and a mode of shading *both*, so that each line of shading on either surface is the polar reciprocal of a line on the other.

16. The conversion of constants into compensative variables may give restricted solutions, as in the ordinary case of two variables, and every other in which the constants are converted into separately self-compensating variables. When these variables are made collectively compensating, and the equations permit elimination of the original variables, ordinary differential equations may be produced, the integration of which may, after substitution, give primitives of the same form as those from which they came. But when the original variables cannot be eliminated, arbitrary relations may be required, in number enough to eliminate the differentials of the new variables: in this case arbitrary functions enter the primitives finally deduced. Of this last case one instance is Lagrange's transition from a primitive of a primordinal partial equation having two constants to the complete primitive of that equation.

17. A biordinal partial equation may be produced from

$$U(x, y, z, a, b, c, e, h)=0$$

by eliminating the five constants between $U=0$ and the five results of primordinal and biordinal differentiation. But it is not true that every form of $U=0$ leads to one biordinal equation only: many forms lead to an infinite number. Two attempts to procure other primitives by making a, b, &c. compensative variables, end in two different forms of result. First, when all the resulting equations are required to be integrable, by introduction of a proper factor, the success of the process requires the integral of two partial equations, one primordinal and one biordinal, between four variables. Secondly, when no such condition is required, the result is another form involving five constants.

18. A primordinal partial equation belongs to a family of surfaces of which one is determined by any given curve through which it is to be drawn. A biordinal equation belongs to an infinite number of families; and a distinct conception of the conditions which select an individual surface is best formed by an extension of the following kind. A curve on a surface is analogous to a point on a curve: two curves being drawn on a surface, the analogue of the chord joining two points on a curve is the developable surface (or surfaces) drawn through the two curves. The developable surface which touches the given surface in a curve (and not the tangent plane) is the ana-

logue of the line which touches a curve in a point. A biordinal equation being given, one surface satisfying it is selected by a curve through which that surface is to pass, and a developable surface passing through that curve which the surface is to touch.

19, 20. The restrictions under which two arbitrary forms must enter, in order that a biordinal partial equation may exist independent of these functions, are wholly unknown. The case which is fully analogous to a biordinal of two variables, is of the most limited character. Ampère has noticed this: Mr. De Morgan was led to it by an examination of the polar properties of $\phi(x, y, z, a, b, c)=0$. This equation leads to $a=\mathrm{A}$, $b=\mathrm{B}$, $c=\mathrm{C}$, where A, B, C are functions of x, y, z, p, q. The primordinal $f(\mathrm{A}, \mathrm{B}, \mathrm{C})=0$ is satisfied by $\phi=0$, subject to $\phi(a, b, c)=0$, and leads to a biordinal, independent of f, of the form

$$\mathrm{Q}+\mathrm{R}r+\mathrm{S}s+\mathrm{T}t+\mathrm{U}(s^2-rt)=0,$$

in which Q, R, &c. are not wholly independent of each other.

If the pole (a, b, c) move along a certain curve, the polar surface must touch a certain surface in one of the lines of a certain shading. That is, every *abc*-curve has a shaded surface, which is its polar reciprocal: and every line of shading of that surface has another surface for its polar reciprocal, shaded by lines of which the original *abc*-curve is one. And every surface has a reciprocal surface such that for each point on one there is a point on the other; and the point on one surface being taken, the polar surface of that point touches the other surface in the other point.

The singular solutions of the two biordinals derived from

$$\phi(x, y, z, a, b, c)=0$$

by means of x, y, z and of a, b, c, are connected by relations analogous to those already seen in the case of two variables. In fact, there is perfect coincidence and coextension between the properties of the general equation $y''=\chi(x, y, y')$ and a particular species of the equation $\mathrm{Q}+\mathrm{R}r+\mathrm{S}s+\mathrm{T}t+\mathrm{U}(s^2-rt)=0$. It is proposed to call this species the *polar biordinal*.

21. The general method of transforming partial equations, given in the last paper, is the investigation of the class of surfaces contained under a given equation by reference of them to their polar reciprocals, any convenient modular equation $\phi(x, y, z, a, b, c)$ being made the means of transformation.

22. The following notation is proposed for eliminants. The components being A_p, A_q, &c., B_p, &c., the eliminants are (A_p), (AB_{pq}), (ABC_{pqr}), &c.; the components being A, A′, &c. B, &c., the eliminants are (A°), $(\mathrm{AB}^{\circ\prime})$, $(\mathrm{ABC}^{\circ\prime\prime})$, &c. Thus

$$(\mathrm{A}_p)=\mathrm{A}_p$$

$$(\mathrm{AB}_{pq})=\mathrm{A}_p(\mathrm{B}_q)-\mathrm{B}_p(\mathrm{A}_q)$$

$$(\mathrm{ABC}_{pqr})=\mathrm{A}_p(\mathrm{BC}_{qr})+\mathrm{B}_p(\mathrm{CA}_{qr})+\mathrm{C}_p(\mathrm{AB}_{qr})$$

$$(\mathrm{ABCD}_{pqrs})=\mathrm{A}_p(\mathrm{BCD}_{qrs})-\mathrm{B}_p(\mathrm{CDA}_{qrs})+\mathrm{C}_p(\mathrm{DAB}_{qrs})-\mathrm{D}_p(\mathrm{ABC}_{qrs}),$$

and so on. Some slight investigation of properties is made, to exhibit the notation.

The following rule is suggested to determine, in any complicated case, whether the number of contiguous interchanges by which one arrangement of letters is converted into another shall be odd or even. This is an important matter in the theory of eliminants, though very complicated instances may seldom occur in practice. Write down one arrangement under the other, and, beginning at one letter in one line, mark the companion letter in the other line, pass on to that companion in the first line, mark its companion, and so on, until we arrive at a letter already marked. Call this sequence a *chain*, each mark being one *link*. Having formed one chain, begin at a letter not yet used, and form another; and so on until every letter has been used. Then, according as the number of chains with *even* links is odd or even, the number of interchanges of contiguous letters required is odd or even. For example, the two arrangements being

A B C D E F G H I J K L M N O P Q

H M O G Q B K L J P F C I N A D E

1 2 1 2 3 2 2 1 2 2 2 1 2 4 1 2 3.

Under A is H, under H is L, under L is C, under C is O, under O is A, already taken: the first chain has five links, the second is found to have nine, the third two, the fourth one. The number having even links is one, an odd number; hence an odd number of contiguous interchanges converts the first arrangement into the second.

23. The following is the method of ascertaining whether the biordinal equation

$$Q+Rr+Ss+Tt+U(s^2-rt)=0 \quad . \quad . \quad . \quad . \quad (1)$$

possesses a primordinal of the form $f(x, y, z, p, q)=0$, containing an arbitrary function. Considering x, y, z, p, q as five independent variables, integrate, by common methods, the equations

$$U\left(\frac{dv}{dx}+p\frac{dv}{dz}\right)+T\frac{dv}{dp}-\frac{k}{1+k}S\frac{dv}{dq}=0$$

$$U\left(\frac{dv}{dy}+q\frac{dv}{dz}\right)+R\frac{dv}{dq}-\frac{1}{1+k}S\frac{dv}{dp}=0,$$

k being one of the roots of $kS^2=(1+k)^2(RT+QU)$. If a common solution $v=A$ can be found, then $A=$const. is a primordinal of (1).

If two common solutions, A and B, can be found, then $B=\varpi A$ is a primordinal, ϖ being arbitrary. But though in this case $A=$const. and $B=$const. are solutions, they cannot *coexist*, unless the values of k be equal, or unless $S^2=4(RT+QU)$. This last equation is one condition of polarity; and if, when satisfied, we find *three* (and there cannot be more) common solutions, A, B, C, inexpressible in terms of each other, then $f(A, B, C)=0$ is the most general primordinal, any two forms of it may coexist, or even any three, which amount to $A=$const., $B=$const., $C=$const. Elimination of p and q between these last equations gives $\phi(x, y, z, a, b, c)=0$, the modular equa-

tion. And the general solution of (1) is found by assuming b and c in terms of a, and then making a a self-compensating variable.

24. The paper is concluded by some remarks on notation.

In an appendix to the preceding paper, read to the Society on the 1st of May, 1854, Mr. De Morgan points out an error committed by M. Cauchy in a very remarkable theorem, of which his enunciation is as follows.

Let ϕx be a function which can be developed in integer powers of x. Let $r(\cos\theta+\sin\theta\,.\,\sqrt{-1})$, r being positive, be any one of the roots of $\phi x=\infty$ or of $\phi' x=\infty$. Then the development of ϕx is convergent from $x=0$ up to $x=$ the least value of r.

M. Cauchy stipulates that the function shall be continuous; but he defines a function to be continuous so long as it remains finite, and receives only infinitely small increments from infinitely small accessions to the variable. It is then obviously impossible that the above theorem should be universally true. Were it so, it would follow that the development of $(1+x)^{\frac{3}{2}}$ is convergent for all finite values of x, whereas it is well known that this development becomes divergent when x is greater than unity. The error of M. Cauchy's demonstration (which contains a valuable method for establishing a large class of definite integrals) is the assumption that if an infinite number of convergent series of the form $a+bx+cx^2+\ldots$, all with one value of x but different values of $a, b, c, \ldots$, be added together, the sum divided by the number of series is also a convergent series. This assumption is not universally true.

Mr. De Morgan takes a totally different line of demonstration, and establishes the following theorems.

If $r(\cos\theta+\sin\theta\,.\,\sqrt{-1})$, r being positive, represent a root of any one of the equations $\phi x=\infty$, $\phi' x=\infty$, $\phi'' x=\infty$, ... then the development of ϕx in powers of x is always convergent from $x=0$ up to $x=$ the least value of r, and divergent for all greater values of x.

If the development have all its coefficients positive, or if all beyond an assignable coefficient be positive, the least value of r is obtained from a real and positive root.

If the signs of the development be, or finally become, recurring cycles, with l in each cycle, the least value of r is obtained from a root in which $\cos\theta+\sin\theta\,.\,\sqrt{-1}$ is one of the lth roots of unity. If no such cycle be finally established, $\cos\theta+\sin\theta\,.\,\sqrt{-1}$ may have a value of θ which is incommensurable with the right angle.

M. Cauchy has established from his own theorem (the want of sufficient statement of conditions not affecting this particular case) the necessity of the observed fact, that the developments produced by Lagrange's theorem for the development of implied functions always give, when convergent, the least of the real values which are implied.

May 1, 1854.

A paper was read by Professor De Morgan on the Convergency of Maclaurin's Series, being an Appendix to a paper on some Points in the theory of differential equations. See the abstract of the former paper, Phil. Mag. vol. vii. p. 450.

Mr. Kingsley made an oral communication on the Chemical Nature of Photographic Processes.

May 15, 1854.

A paper was read by Mr. Warburton on Self-repeating Series.

In computing Bernoulli numbers by the formula of Laplace*, the author of this paper was led to notice, that in the fraction whose development is a series of the form $1^{2n+1}-2^{2n+1}t+3^{2n+1}.t^2-$&c., the numerator of that fraction is a recurrent function of t. This led him to investigate the question, what are the conditions which the denominator of the generating fraction, and the terms of the series generated, must satisfy, in order that the numerator of such a fraction may be a recurrent function of t. The paper contains the result of that investigation.

The author calls those series "*self-repeating*," which, when extended without limit in opposite directions, admit of separation into two similar arms, each arm beginning with a finite term of the same magnitude. Between this pair of finite terms, either no zero-term, or one or more zero-terms, may intervene. One arm repeats, and contains arranged in reverse order, the terms of the other arm, either all, or none, of the terms having their signs changed. The different positive integer powers of the natural numbers, of the odd numbers, and of the figurate numbers of the several orders, present familiar examples of self-repeating recurring series.

The author demonstrates the following three theorems respecting self-repeating recurring series:—

I. If the series arising from the development of a proper fraction is the right arm of a self-repeating recurring series, and if the denominator of such a fraction is a recurrent function of t, then the numerator also is a recurrent function of t.

II. Other things remaining the same, if the numerator of the fraction is a recurrent function of t, then the denominator also is a recurrent function of t.

III. If the numerator and the denominator of a proper fraction are each a recurrent function of t, then the series, arising from the development of the fraction according to the positive integer powers of t, will be the right arm of a self-repeating recurring series.

By way of example, the author applies his first theorem to the summation of the infinite series $1^9-2^9+3^9-$&c., and compares his process with the corresponding processes of Laplace and of Sir John Herschel. The sum in question is given by Sir John Herschel (see Jameson's Journal, January 1820) in terms of the differences of the powers of 0, extending from $\Delta^{10}0^9$ to $\Delta^{90}0^9$. In the author's process,

* See Memoirs of the Academy of Sciences, 1777.

the requisite differences extend from $\Delta^{10}0^{0}$ only to $\Delta^{5}0^{0}$, and the numerical coefficients of these are of diminished magnitude, and of very easy determination.

The author makes other applications of his theorems; but on these we forbear to enter.

A paper was read by Professor Challis on the Determination of the Longitude of the Cambridge Observatory by Galvanic Signals.

The experiment of which this paper contains the details, was made at the suggestion of the Astronomer Royal, and conducted according to a scheme arranged by him for giving and receiving the signals. A galvanic connexion having been established between the Greenwich Observatory and the Cambridge Telegraph Office, by means of the London central station of the Electric Telegraph Company, signals were sent on the nights of May 17 and 18, 1853, between 11^{h} and 12^{h} mean time. The signals were made by causing two needles, one at Greenwich, the other at Cambridge, to start by completing the galvanic circuit at either place of observation. The times of starting were noted at both places, and reduced to the sidereal times of the respective observatories, to serve by comparison for determinations of the difference of their longitudes. On each night the signals were made alternately for a quarter of an hour at one station, in batches containing an arbitrary number of signals not exceeding nine, and then for a quarter of an hour at the other station in a similar manner. On the first night the total number of signals was 151, and on the second night 139. The two observers, Mr. Dunkin of the Greenwich Observatory, and Mr. Todd of the Cambridge Observatory, changed places in the interval between the two nights' observations; Mr. Todd observing at Greenwich, and Mr. Dunkin at Cambridge, on the second night. Also it was arranged that the two observers should observe identical stars on the two nights, as well as the stars ordinarily used for clock errors, and that the same apparent right ascensions of the stars should be employed for reducing the signal-times at both observatories. The Cambridge Observatory time was conveyed with the greatest care to the Telegraph Office at the Cambridge Railway Station by the transfer of three chronometers. By a first calculation, the longitude of the Cambridge Observatory was found to be $23^{s}{\cdot}03$ east of Greenwich.

Professor Challis subsequently made another calculation, taking into account the effect on the times of meridian transits of stars produced by the forms of the transit-pivots, according to a method which he has described in the Memoirs of the Royal Astronomical Society (vol. xix. p. 103). The errors arising from the deviation of the pivots from the cylindrical form being eliminated, the longitude is found to be $22^{s}{\cdot}70$ east of Greenwich, which is less by $0^{s}{\cdot}84$ than the value hitherto adopted.

May 29, 1854.

A paper was read by Professor Fisher, entitled "Additional Observations on the Development of the Vertebral System."

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

November 13, 1854.

A paper, by R. L. Ellis, Esq., was read, entitled "Remarks on the Fundamental Principle of the Theory of Probabilities."

Also, "On the Purbeck Strata of Dorsetshire." By the Rev. O. Fisher.

The object of this paper was to describe the beds from which a series of insect remains and other fossils had been collected by the author, and presented to the Woodwardian Museum.

The connexion of the Purbeck beds with the Oolitic rather than with the Wealden series was maintained, while both were shown to be unconformable in this district to the cretaceous system. Reasons were given for thinking that the materials, of which both the Wealden and Purbeck were composed, had travelled from west to east; and the beds of the New Red Sandstone, as they occur in Devonshire, were pointed out as affording a mass of strata which would furnish a detritus of the character of a large portion of the Hastings sands of Hampshire and Dorsetshire.

In describing the Purbeck beds, the author followed the system of the late Professor E. Forbes, dividing them into upper, middle, and lower; and entered into some detail of the alternations of salt and freshwater conditions that prevailed during their deposition. The aspects under which the same beds appear at different points of the district under examination were particularized, and it was attempted to be shown that these were in conformity with the theory of a current setting from the west towards the east. The mode of occurrence of the remains of insects in the middle and lower Purbecks was somewhat minutely described, and it was suggested that some interesting chronological speculations might be grounded upon it.

The paper concluded with an attempt to explain the singular fractured condition of about thirty feet of the lower Purbeck strata throughout the eastern part of the county. It was supposed that this might have been caused by the deposition of sediment upon the remains of the Portland forest before the mass of the trees had been removed by decomposition; the sediment, after it had become consolidated, settling unequally as the carbonaceous matter was gradually removed.

November 27, 1854.

Prof. Willis gave an account of a new form of Atwood's Machine.

December 11, 1854.

A communication was made by Dr. Paget on a case of involuntary tendency to fall forwards.

February 19, 1855.

Mr. Hopkins gave a lecture on certain changes of Terrestrial Temperature, and the causes to which they may be attributed.

March 5, 1855.

Dr. Clark gave an account of some recent discoveries respecting the origin, migrations, and metamorphoses of Entozoa, and their bearing on the notion of spontaneous generation.

April 23, 1855.

A paper was read by the Master of Trinity, on Plato's Survey of the Sciences, contained in the seventh book of the Republic.

Plato, like Francis Bacon, took a review of the sciences of his time; and like him, complained how little attention was given to the philosophy which they involved. The sciences which Plato enumerates are arithmetic and plane geometry, treated as collections of abstract and permanent truths; solid geometry, which he "notes as deficient" in his time, although, in fact, he and his school were in possession of the doctrine of the "five regular solids;" astronomy, in which he demands a science which should be elevated above the mere knowledge of phænomena. The visible appearances of the heavens only suggest the problems with which true astronomy deals; as beautiful geometrical diagrams do not prove, but only suggest geometrical propositions. Finally, Plato notices the subject of harmonics, in which he requires a science which shall deal with truths more exact than the ear can establish, as in astronomy he requires truths more exact than the eye can assure us of. It was remarked also, that such requirements had led to the progress of science in general, and to such inquiries and discoveries as those of Kepler in particular.

May 7, 1855.

The Master of Trinity read a paper on Plato's notion of *Dialectic*.

At the end of the survey of the sciences contained in the seventh book of the Republic, which was the subject of a paper at the last meeting, Plato speaks of *Dialectic* as a still higher element of a philosophical education, fitted to lead men to the knowledge of real existences and of the Supreme Good. Here he describes Dialectic by its objects and purpose. In other places Dialectic is spoken of as a method or process of analysis; as in the 'Phædrus,' where Socrates describes a good dialectician as one who can divide a subject according to its natural members, and not miss the joint, like a bad carver. Another Dialogue, in which there are examples given of dividing a subject, is the *Sophistes*, where many examples of dichotomous or bifurcate division are given. But this appears from the Dialogue to have been a practice of the Eleatic rather than of the Platonic school. Aristotle proposed a division of subjects according to his ten *Categories*, which he and others since have extensively used. Xenophon says that Socrates derived *Dialectic* from a term implying to *divide a subject into parts*, which Mr. Grote thinks unsatisfactory as an etymology, but which has indicated a practical connexion in the Socratic school. The result seems to be, that Plato did not establish any method of analysis of a subject as his Dialectic; but he conceived that the analytical habits formed by the comprehensive study of the exact sciences, and sharpened by the practice of dialogue, would lead his students to the knowledge of first principles.

Also, Mr. Maxwell gave an account of some experiments on the mixture of colours.

May 21, 1855.

A paper was read by Mr. Hopkins on the External Temperature of the Earth and the other Planets of the Solar System.

We have not sufficient *data* to determine the superficial temperature of any planet besides our own. We know, however, that it must mainly depend on the temperature of the planetary space, and on the heat which the nearer planets at least receive directly from the sun, but modified, and possibly in a far greater degree than has been generally supposed, by the particular circumstances by which each planet may be characterized. The modifying circumstances more particularly referred to in this paper, are the existence of atmospheres surrounding the planets, the positions of their axes of rotation, and the conductivity and specific heat of the substances forming the outer crust of each planetary body of our system. No astronomer, judging from the appearances which Mars and Jupiter present to us, would entertain any serious doubt as to the existence of atmospheres surrounding those planets, and the probability would seem to be almost equally strong of Saturn being likewise enveloped

in a similar manner. The obliquity of the axis of rotation is known with considerable accuracy in the cases of Mars and Jupiter; and also in that of Saturn, if it coincide with the axis of rotation of his ring. Venus presents great difficulties to the observer, but it appears now to be pretty satisfactorily determined, that the period of rotation about her own axis is nearly the same as that of the Earth, and that the obliquity of her axis is large, amounting to as much as about 75°. This must produce an extraordinary difference between the changes of *annual* temperature in that planet and those which we experience. The author has endeavoured, in this paper, to estimate numerically the effect of this anomalous obliquity. Practical astronomers have entertained the opinion that Venus likewise has an atmosphere. Of Mercury we know too little by direct observation to form any opinion on those points founded on observed facts, and the same remark will apply to the remoter planets beyond Saturn; but most astronomers probably feel much the same conviction that Mercury, Uranus, and Neptune have atmospheres of greater or less extent, as that they revolve round their own axes with greater or less angular velocity.

It is not the author's object, however, to adjust the balance of probabilities for particular hypotheses in favour of planetary atmospheres or against them; but assuming their existence, to estimate their effects on the planetary temperatures. And in like manner he points out the influence which must be exercised by a greater or less conductivity, and specific heat in the superficial matter of a planet, without professing to discuss the probability of such properties being materially different in the different planets. The Earth's atmosphere is known to be almost completely diathermanous for heat radiating directly from the Sun; and it is assumed to be equally so for the heat which proceeds directly from the fixed stars, and to which the general temperature of space is due. This radiating heat therefore has little or no effect in heating the atmosphere during its transmission to the Earth's surface; but after falling upon and heating terrestrial objects, it loses the power of radiating completely through the atmosphere, and is transmitted back into space through the atmosphere by *conduction, convection,* and *partial radiation* to limited distances. But for any of these modes of transmission, it is essential that the temperature of the atmosphere should be greater in its lower than in its upper portions, and in a degree greater as the quantity of heat to be transmitted is greater. The temperature (τ_2) of the upper portion must be determined by the condition, that, in a given time, a quantity of heat must radiate from it into surrounding space equal to that which falls upon it from external sources in the same time, and is transmitted back after reaching the surface of the earth or objects near to it. Consequently τ_2 must be independent of the height of the Earth's atmosphere. At lower points the temperature will increase till we reach the surface of the Earth; and if we denote the temperature there by τ_1, it is manifest that τ_1 will be greater, the greater the height of the Earth's atmosphere.

It must here be particularly observed, that τ_2 is the proper tem-

perature of the component particles of the atmosphere, and is probably widely different from the temperature which would be indicated by a thermometer placed at the upper extremity of the atmosphere, since the instrument would not only be affected by the exchange of heat between its bulb and the atmospheric particles, but also by the heat radiating upon its bulb from every source of heat in surrounding space; while the atmosphere, on account of its diathermancy, would remain unaffected by this radiating heat.

Conceive now a thermometer to be placed at a point sufficiently above the earth's atmosphere. If the bulb were sheltered from the direct influence of the solar rays, the thermometer would indicate the temperature of that point of space, independent of the effect of radiation from the central luminary of the solar system, but dependent on the radiation from all other sources of heat in the universe. If the instrument thus sheltered were sufficiently remote from the sun and every planet, it would indicate very nearly the same temperature at every point within the solar system, assuming the absence of all unknown centres of heat within that system or near to it. This is what may be understood by the general temperature of planetary space. Let it be denoted by T. We shall then have T greater than τ_2; and therefore if we now conceive the thermometer to be transported to the upper limit of the atmosphere, it will be affected by the lower temperature there, and will indicate a temperature intermediate to T and τ_2. If the instrument be brought still lower within the atmosphere, it will indicate a still lower temperature, from its being entirely surrounded by a portion of the atmosphere more dense than that at the extreme boundary, till this tendency to lower the indications of the thermometer is counteracted by the greater temperature of the atmospheric particles as we descend towards the Earth's surface. At some point, consequently, within the Earth's atmosphere the indication of the thermometer would attain its *minimum*; after which, in descending continuously towards the Earth, the temperature indicated would constantly increase, omitting variations due to temporary or local causes. Thus it follows that the existence of an atmosphere like that of the Earth, enveloping a planet, may, according to its extent, either elevate the superficial temperature of the planet above, or depress it below that of surrounding space independently of the direct solar radiation. With respect to our own globe, we are entirely ignorant of the height to which the thermometer, in ascending, would continue to indicate a decreasing temperature, but we are sure that such height is great. This is important with reference to the ultimate object of this paper; for if the height of a planet's atmosphere were too small to allow a thermometer descending in it to attain its *minimum* indication, it is manifest that an increase of atmosphere would cause a *decrease* in the planet's superficial temperature; whereas if the height of the atmosphere were great enough to allow the thermometer to attain the *minimum*, any increase of atmosphere would necessarily cause an *increase* in the superficial temperature of the planet. In the Earth's atmosphere, we are sure (as just remarked) that the indications of a thermometer

would constantly increase in its descent from a very high point above the Earth's surface; and therefore it follows, that if a planet be enveloped in an atmosphere similar to that of the Earth, but of greater height, the superficial temperature of that planet will be higher than that of the Earth, supposing both to exist in the planetary space unaffected by the heat which radiates from the Sun; while, on the contrary, the superficial temperature of the planet would necessarily be less, under the same conditions, than that of the Earth, if its atmosphere were smaller, unless it should be so small as not to allow a thermometer descending in it to reach its *minimum* indication. If the planet were entirely without atmosphere, its superficial temperature (in the assumed absence of solar radiation) would be that of surrounding space; but we have no means of determining what relation that temperature bears to existing terrestrial temperature, or to what this latter temperature would become in the absence of solar radiation.

The author has calculated from Poisson's formulæ the increase of temperature in the superficial crust of the Earth, due to the amount of heat received by direct radiation from the Sun, in different latitudes, above that temperature which would be common to all parts of the Earth's surface in the absence of solar radiation, and with a uniformity of intensity of stellar radiation in all directions upon our globe. But this increased temperature must produce an augmentation of temperature in the atmosphere, which must react on the terrestrial temperature till equilibrium of temperature be established. The author has endeavoured to estimate the amount of this indirect effect of solar radiation by means of the data furnished by M. Dove's work on terrestrial temperatures, combined with calculations based on Poisson's formulæ. He concludes that the whole effect of solar heat at any proposed place is very nearly double that due to the immediate and direct effect of solar radiation. Having thus ascertained this entire effect, he finds the temperature which would pervade the whole surface of the earth if the solar heat were extinguished. He estimates this temperature at $-39^{\circ}\cdot5$ C.

The *annual variation* of temperature in any latitude is found to be nearly the same in amount for the terrestrial surface and for the part of the atmosphere resting upon it. This must be understood as applying to those places at which the temperature is not materially affected by the *horizontal* transference of heat by marine or aërial currents, or any local causes, which disturb the dependence of temperature on latitude alone. The author also points out the dependence of the annual inequalities of the terrestrial temperature (and consequently of those also of the atmosphere) on the conductivity and specific heat of the matter which constitutes the Earth's crust. If these were much greater, the annual changes of temperature would be much less.

Before applying these results to other planets, the author states that he does not admit the notion, that the remoter planets may derive a considerable superficial temperature from the remains of that internal heat which they probably possessed in the earlier stages of

their existence. It is a well-established conclusion, that the superficial temperature of our own globe has arrived at that point below which it can never descend by more than the small fraction of a degree, so long as all *external* conditions remain the same as at present; and the superficial temperature of the remoter planets will in all probability be reduced to the corresponding limit. To these external conditions, therefore, and not to their primitive heat, must the existing temperatures on the surfaces of these planets be attributed, assuming always that they are not of less antiquity than our own globe. Hence the superficial temperature of the Earth, with its present atmosphere, placed at the distance of Neptune, Uranus, or Saturn, would be very nearly $-39^{\circ}\cdot5$ C., since the effect of solar radiation at those distances would be nearly insensible. But if the extent of the atmosphere were increased, the superficial temperature would be augmented in a corresponding degree. Judging by the decrements of temperature observed by Mr. Welsh, the author concludes that an increase in the height of the Earth's atmosphere of 35,000 or 40,000 feet, would elevate her superficial temperature, if placed in the remoter planetary regions, to nearly the mean temperature of our present temperate zone. The same conclusion will hold with respect to the three planets above mentioned, if we suppose them to have atmospheres similar to that of the Earth, and of sufficient extent. Their temperatures must be sensibly uniform over the whole of their surfaces, not being subject to any appreciable *annual* variation.

The same conclusions will apply to Jupiter, except that there will be a small augmentation of temperature arising from solar radiation, which the author calculates might amount to about $2\frac{1}{2}^{\circ}$ C. at his equator.

Hence the author concludes that those views which assign a necessarily low temperature to the above-mentioned planets in consequence of their distance from the Sun, are altogether untenable.

The conditions under which Mars is placed approximate more nearly to those of the Earth than for any other planet. The author calculates, that with an atmosphere similar to that of the Earth, and exceeding it in height by about 15,000 or 20,000 feet, the equatorial temperature of Mars may be about 60° F., or $15^{\circ}\cdot5$ C., and his polar temperature about -10° C. The extent of the *annual* variations would be about half those on our own planet in corresponding latitudes, supposing the conductivity, specific heat, and radiating power of the matter composing his superficial crust to be the same as for the Earth.

Again, if the Earth, with her present atmosphere and *obliquity*, were placed in the orbit of Venus, the mean equatorial temperature would be upwards of 90° C., subject to the reduction, which would doubtless in this case be great, due to the horizontal transference of heat. The mean polar temperature would be about 16° C. A diminution in the atmosphere would reduce these temperatures in any assigned degree. But the *obliquity* of Venus, though not satisfactorily determined, is considered to be much greater than that of

the Earth, amounting, according to the estimate of some astronomers, to as much as 75°, as heretofore stated. This would of course render the character of her seasons entirely different from those of the Earth. The greatest mean annual temperature would be at the pole. Independently of the horizontal transference of heat by aërial currents or other causes, taking the extreme obliquity of 75°, and supposing the atmosphere of Venus to be exactly like that of the Earth, her mean temperature at the equator would be about 56° C., and at the pole 95° C. This latter would probably be much lowered by currents; but if the height of the atmosphere of Venus be less than that of the Earth's atmosphere by about 25,000 feet, the author considers that the mean temperature of Venus in her equatorial regions would not exceed that of the temperate regions of the Earth; while the mean polar temperature would probably be about 40° C., or about 12° or 13° C. higher than the Earth's equatorial temperature. The heat of *sunshine* may be moderated by an atmosphere more laden with vapour than that of the Earth.

Supposing the atmosphere of Venus like that of the Earth in its nature and its magnitude, the temperature at her poles, with the supposed obliquity, must be subject to an enormous *annual* inequality, amounting to between 70° and 80° C. above or below the mean temperature, liable, however, to a great reduction by horizontal transference of heat. It may also be considerably reduced by the nature of the matter which constitutes her outer crust. A reduction, likewise, in the extent of her atmosphere, like that above supposed, would probably diminish the amount of this inequality, as well as the mean temperature, though not in the same degree. It is easy to conceive that the coefficient of the inequality may be thus reduced to some 40° C.; and supposing the mean temperature then, as above estimated, at about 40° C., the *annual* polar temperature will oscillate between 0° C. and 80° C. At the equator, the *semi-annual* inequality might amount, under the above suppositions, to about 10° or 12° C., in which case the equatorial temperature might oscillate between something below zero (C.) and some 25° C. It should be recollected also, that a much greater reduction of the mean temperature would result from a greater reduction in the extent of this planet's atmosphere than above supposed with reference to the height of our own atmosphere. This would not, the author conceives, be inconsistent with the existence of a large quantity of vapour in the atmosphere, affording shelter from the heat and glare of sunshine.

The Moon is under the peculiar circumstances of the absence of a sensible atmosphere, and her long period of rotation about her axis. Assuming her to have no atmosphere at all, the mean temperature of her outer crust, in the absence of the Sun, would be the general temperature of that portion of planetary space in which the solar system is situated. How much this might differ from the superficial temperature which the Earth would have with the like absence of the Sun, and which the author estimates at $-39^{\circ}\cdot5$ C., as above stated, it is impossible to determine; but whatever it may be, the influence of the Sun's heat would be to increase it by about

40° C. at the Moon's equator, and by a small amount only at her poles. This must be attended by an enormous monthly inequality, amounting to nearly 60° C., supposing the matter of which her superficial crust is composed to have the same conductivity, specific heat, and radiating power as the crust of the Earth. If these be much greater for the Moon, this inequality might be considerably diminished. At the poles it must be comparatively small.

The lunar temperatures here spoken of are those of the matter forming her external crust. The temperature which would be indicated by a thermometer placed in her immediate vicinity would be affected by the Moon (in the assumed absence of an atmosphere) only by her direct radiation. We have not the means of determining what this temperature may be.

Also a paper was read "On the singular Points of Curves." By Professor De Morgan.

Mr. De Morgan defines a curve as the collection of *all* points whose co-ordinates satisfy a given equation; and contends for this definition as necessary in geometrical *algebra*, whatever limitation may be imposed in algebraic *geometry*. He divides singular points into points of singular *position* and points of singular *curvature*; the character of the former depending on the axes, but not that of the latter. Both species are defined as possessing a notable property, and such as no arc of the curve, however small, can have at all its points.

The form first considered is that of which the case usually taken is an *algebraic* curve. Let $\phi(x, y)$ be a function which for all real and finite values of x and y is real, finite, and *univocal*; let the curve be $\phi(x, y)=0$, considered as an individual of the family $\phi(x, y)=\text{const.}$ The two curves $d\phi : dx=0$, $d\phi : dy=0$, or $\phi_x=0$, $\phi_y=0$, are the *subordinates* of this system, on which the singular points of all depend.

When ϕ is not reducible to another function of the same kind by extraction of a root, it divides the plane of co-ordinates into *regions* in which, severally, it is always positive or always negative. By this consideration it is easily shown (independently of y', y'', &c.), that if $(x+dx, y+dy)$ be a point on the tangent at (x, y), $\phi(x+dx, y+dy)$ has the sign of $\phi_{xx}dx^2+2\phi_{xy}dxdy+\phi_{yy}dy^2$. Hence, immediately after leaving the curve, ϕ agrees with or differs from $-\phi_y y''$ at the point left, according as the curve is left on the convex or the concave side. Hence easily follow the criteria of flexure, and also the following relation between any two points whatsoever of the curve.

Let two points be called *similar* when a line drawn from one to the other cuts the curve an even number of times (0 included) with the same *abutments* (on convexity or on concavity), or an odd number of times with different abutments. Let other points be called *dissimilar*. These points are similar or dissimilar, according as their values of $\phi_y \,.\, y''$ agree or differ in sign.

An *à priori* proof is given that multiple points, cusps, and isolated points, must be determined by $\phi_x=0$, $\phi_y=0$, or can only take place when both subordinates meet the curve. It is shown that, in the

system $\phi(x, y) =$ const., the cusp of $\phi(x, y) = 0$ must be an evanescent loop, and the isolated point an evanescent oval, or bounded portion. Some discussion of the meaning of $y' = a + b\sqrt{-1}$ at an isolated point is given.

There have been two methods of treating the singular points. The first has recourse to the theory of equations, using differentiation, if at all, only to supply coefficients. The second attempts canonical forms derived from differential coefficients, and examines, in succession, the meaning and bearing of the successive orders of differential coefficients. Mr. De Morgan affirms that this second method cannot be what it pretends to be; and, by treating it generally, shows that its questions are ultimately dependent upon the theory of equations. An equation of the form $\Sigma A y'^{x} = 0$, when it has no equal roots, decides the character of a singular point definitively; and reduces it to a number of intersecting branches without contact, a number of coinciding isolated points without real tangents, or some of one and some of the other. When the equation has some real roots, each set furnishes either multiple branches with contact, or cusps, or conjugate points with real tangents. All this is easily illustrated by examining the curve in which $\phi(x, y)$ is an infinitely small constant, near to the singular point of $\phi(x, y) = 0$.

A theorem given by Lagrange, and strongly indicated in the writings of Newton, Taylor, Stirling, Cramer, and John Stewart, but apparently nearly forgotten, solves the question of finding the higher or lower degrees of all the roots of $\Sigma A y^{a} = 0$, where A is a function of x of the degree a; that is, where $A = x^{a}(a + A')$, and A' vanishes when $x = \infty$ or when $x = 0$. By this theorem (which is also given in the first* Number of the Quarterly Journal of Mathematics), y being $x^{r}(u + U)$, all the values of r, and their corresponding values of u, are very easily found; and repetition of the process upon a transformed equation gives $U = x^{r_{,}}(u_{,} + U_{,})$, and so on. It obviously follows, that when the origin is removed to any singular point of a curve, the discussion of the branches which pass through that point, and of their contacts with the tangent and each other, is made very easy. In proof of this, the author takes the following instance,—

$$x^{12} + x^{14} + x^{11}y - x^{8}y^{2} + 2x^{7}y^{3} - x^{4}y^{4} + y^{6} - 3xy^{3} + x^{14}y^{13} = 0,$$

and discusses its infinite branches, and the sextuple point at the origin (which turns out to be a couple of isolated points, and a cusp of similar flexures), with very much less space and trouble than ordinary methods would demand from a much less complicated instance. It is also shown that the *lower form* of Lagrange's theorem solves the following question:—Given an equation with a certain number of equal roots, what effect will be produced upon these roots by given infinitesimal alterations in the coefficients, how many will remain real, and how many will become imaginary?

Newton has given the foundation and the chief step of a geome-

* There attributed to Mr. Minding, by a mistake caused by M. Serret, who incorporates it with a theorem of Mr. Minding, without any notice of its author.

trical method (*Newton's parallelogram*) which has passed into oblivion, though it occurs in the celebrated second letter to Oldenburg, has been fully described by Stirling, used by Taylor and De Gua, and forms the main method of Cramer's work on curves. Mr. De Morgan proposes to call it the method of *co-ordinated exponents*.

He proceeds to describe and enlarge this method; observing that, of the polygon which represents an equation, Newton and his followers are in full possession of the connexion of the sides with the solutions, and fail only in not grasping the connexion of the whole polygon with the whole equation. Both Newton's method and Lagrange's, the second of which is an arithmetical version of the first, may be applied to irrational equations, but it will be convenient to confine the description to the form $\Sigma ax^m y^n = 0$, where m and n are integers.

In $ax^m y^n$, let n be an abscissa, and m an ordinate, and let (m, n) be called the exponent point of the term $ax^m y^n$. Take some paper ruled in squares (or ruled both ways in any manner, for any equal rectangles will do) to facilitate the process when n and m are always integers, and lay down all the exponent points in $\Sigma ax^m y^n = 0$. Through some of these points draw a *convex* polygon including all the rest, which can only be done in one way. Should the points be so many and so scattered that some *method* must be applied, the geometrical method is a translation of the main arithmetical method of Lagrange's theorem. The points which end on, or otherwise fall in, the sides of the polygon show the *essential* terms of the equation: no others are wanted to determine q and u in $y = x^r(u + \mathrm{U})$. The upper contour of the polygon shows how all the solutions commence in descending powers of x; the under contour does the same for ascending powers. Take any side of either contour, its projection on the axis of n shows the number of roots it represents, the tangent of the angle it makes with the *negative* side of the axis of n shows the value of r.

It will not be needful to abstract the developments given in the paper: we shall only notice the *inverse* method. The following example is taken, and the construction of the equation is even easier (under Cramer's form) than the direct treatment of it. The example chosen by the author is the following:—Required $\phi(x, y) = 0$, of the twelfth dimension in terms of y, such that the twelve roots of y, with reference to lower degrees, shall be as follows: two roots of the degree 1, four of $\frac{1}{2}$, two of 0, one of -1, two of $-\frac{3}{2}$, one of -2. But with reference to higher degrees, there are to be one root of the degree 3, two of $\frac{1}{2}$, three of 0, three of $-\frac{1}{3}$, two of -1, one of -3. On examination these conditions are found compatible, and the most general equation which satisfies the conditions is found.

The paper is terminated by a discussion on the pointed branch, for the admission of which, as a branch altogether composed of singular points, the author contends.

November 12, 1855.

A paper was read by the Master of Trinity on the Intellectual Powers according to Plato.

Also, Prof. Sedgwick gave a lecture on the Classification and Nomenclature of the Palæozoic Rocks.

November 26, 1855.

A paper was read on the Earthquake in Switzerland in July last, by the Rev. O. Fisher.

The 25th of July, 1855, on which the first and most severe shock was felt, was a very wet, close day, and the little wind stirring came from the S.W.

In the Münster Thal the earthquake began by a rumbling vibration like that caused by a carriage run under an archway, gradually increased for about four seconds, and then suddenly ceased. The oscillation seemed to be from E.S.E. to W.N.W., but would be affected by the build of the house.

In the church at Bienne two stones fell from the groining thirty or forty feet into the organ pipes, to a point between 2 and $2\frac{1}{2}$ feet N. by E. of the point vertically beneath their first position; and allowing for the direction of the building, this would give the motion of the earth about from N.E. to S.W. This wave may have been a reflexion caused by the wave entering the Jura from the valley. Another shock was felt at Bienne, at 10 A.M., on the 26th.

The great shock was felt at Strasburg, slightly at Lyons in a direction from E. to W.; likewise at Chambery, Alessandria, and Genoa. The account given by Plana in the *Times* does not seem very intelligible, but as far as can be made out from the stopping of the clocks, it gives the direction of the shock at Turin about 30° W. of S. Chiavenna, the western shore of the Lake of Constance, and Schaffhausen seem to fix the limit to which it was felt towards the east. The area shaken was therefore an oval, having its largest dimension about 300 miles N. and S., and its shortest 250 miles E. and W.

At Geneva the shock appeared to be directed to E.N.E. At Thun it appeared to come from Frutigen. At Kandersteg, at the north foot of the Gemmi, the shock was N. and S. At Interlaken the shocks were more severe; and at Ormont, Canton Vaud, the oscillation came from W. to E., preceded by a noise which lasted for an instant only, and the roof of a house fell in. It seems that nearer the centre of the oval the intensity of the shock was greater. At the baths of Leuk a chimney was thrown down and the walls cracked; but on ascending the valley of the Rhone the evidence of disturbance became rapidly more marked up to Visp, where only seven houses remained habitable. At the little inn, the "Soleil," the flag pave-

ment was burst upwards as if by a blow from beneath: a continual succession of shocks have occurred there at variable intervals up to the present time. Passing on towards Brieg, the evidence of the violence of the shock rapidly diminished. The valley of Zermatt showed the chief disturbance; the bridle road was continually fissured, and in some places slipped down into the valley. At Stalden there was much destruction, but at St. Nicholas the havoc was very great indeed. Higher up the traces of the shock were less and less, until at Tesch, Randa, and Zermatt, there was no mischief done. The other branch of the valley by Saas did not suffer so much.

Drawing lines through the different places in the direction in which the wave proceeded, it will be found that they converge very nearly to Visp, showing that to be nearly the centre of disturbance.

Mr. Croker of Caius College was walking between Stalden and Visp when the great shock occurred, which appeared to him to be a blow from beneath like the springing of a mine under him, and he observed that the path sunk several inches from the solid rock; a lofty isolated rock on the opposite side of the valley vibrated, and blocks of stone came tumbling down on all sides. The quivering lasted about thirty seconds. He did not observe any sound preceding the shock, though this was heard at Visp; but a crashing sound accompanied the great shock, and a fainter sound continued afterwards beyond the motion. He felt continued shocks from one o'clock till four, when he proceeded towards Sion. At Zermatt the same shock was felt very much less violently, and no sound preceded it; and after attaining its maximum, it ceased somewhat suddenly. It was felt less strongly on the Riffelberg; and on the 27th another, felt at Zermatt as strongly as before, was not felt on the little Mont Cervin.

A sound seems in general to have preceded the earthquake at places near the centre of disturbance: at Visp likened to the echo of an avalanche, but at a distance there was only a sound simultaneous with the shock. The sound may have arisen from the grinding of the walls of the fissure, or whatever violent action may have occurred at the origin, and the sound-waves travelling more rapidly than the earthquake-wave. This is opposed to Mr. Mallet's view, though he gives a table in which the least rate given for sound travelling through any kind of stone is 3640 feet per second, while the rate of motion of the earthquake of Lisbon was 1750 feet. If the view stated be correct, the disturbance must have been deep in the earth, which would also explain the upward blow felt by Mr. Croker. At greater distances the sound-wave would be expended sooner than the earthquake-wave, and the accompanying sound be due to local action.

Chimneys and such like structures appear to have fallen away from the centre of disturbance, being thrown down by the return stroke of the wave; the forward stroke having to move them only from a state of rest, whereas the return stroke would have to overcome the momentum generated by the former.

Near the centre the shock was sudden, passing away gradually.

At a distance it began with slight quivering, gradually attained a maximum, and then suddenly ceased. Now if the disturbance occurred along a large fissure, perhaps several miles in length, and of unknown depth, the waves from different portions would reach any given point in succession, and at intervals the combined effect of many waves would be felt, producing a result analogous to the rolling of thunder due to the varying distance of the source of sound, while the sudden concussion at a nearer point is like the detonation heard when the lightning is near the auditor.

The shocks were less severe in the mountains than in the valleys. As far as the wave progressing horizontally is concerned, it would, on entering a mountain, at first be nearly bounded by a horizontal plane continuous with that of the valley, just as light is propagated in straight lines; but there would also be a diversion of a part (analogous to the diffraction of light at a screen) into the mountain, so that where the wave passed for some distance into a range it would finally be felt at the summit. It is observable that the shock on the 25th was less severe on the Riffelberg than at Zermatt, yet it travelled through the mountain and was felt at Turin.

The period of elevation of the Alps seems about contemporaneous with the older Pliocene of Sir C. Lyell. The country is broken up with faults, which probably there, as elsewhere, follow the lines of valleys. The valley of Visp lies in the axis of two ranges which have all the appearance of a mighty valley of elevation. The shock may have arisen from a shifting of the beds on this line of ancient disturbance, and very probably the somewhat rectangular corner between the valleys at Visp suffered the principal displacement. Earthquakes in non-volcanic regions probably arise from a failure of support. At the period of the elevation of the Alps, the more heated lower parts of the earth's crust must have come nearer to the surface than their normal position, and contractions and failure of support must occur while cooling, and the comparatively recent elevation of the Alps may give reason for thinking this to be still going on.

December 10, 1855.

A paper was read by Mr. Maxwell on Faraday's Lines of Force.

The method pursued in this paper is a modification of that mode of viewing electrical phænomena in relation to the theory of the uniform conduction of heat, which was first pointed out by Professor W. Thomson in the Cambridge and Dublin Mathematical Journal, vol. iii. Instead of using the analogy of heat, a fluid, the properties of which are entirely at our disposal, is assumed as the vehicle of mathematical reasoning. A method is given by which two series of surfaces may be drawn in the fluid so as to define its motion completely. The uniform motion of an imponderable and incompressible fluid permeating a medium, whose resistance is directly as the velocity, is then discussed, and it is shown how a system of surfaces of equal pressure

may be drawn, which, with the two former systems of surfaces, divides the medium into cells, in each of which the same amount of work is done in overcoming resistance. It is then shown that if the fluid be supposed to emanate from certain centres, and to be absorbed at others, the position of these centres can be found when the pressure at any point is known; and that when the centres are known, the distribution of pressures may be found. Methods are then given by which the motion of the fluid out of one medium into another, the resistance of which is different, may be conceived and calculated, and the theory of motion in a medium in which the resistance is different in different directions is stated.

The mathematical ideas obtained from the fluid are then applied to various parts of electrical science. It is shown that the expression for the electrical potential at any point is identical with that of the pressure in the fluid, provided that "sources" of fluid are put instead of positive electrical "matter," and centres of absorption or "sinks" for negative "matter."

The theory of Faraday with respect to the effect of dielectrics in modifying electric induction, is illustrated by the case of different media having different conducting power; and it is shown, that, in order to calculate the effects by the ordinary formulæ of attractions, we must alter in a certain proportion the quantities of electricity within the dielectric, and conceive an imaginary distribution of electricity over the surface which separates it from the surrounding medium.

The theory of magnets and of the phænomena of paramagnetic and diamagnetic bodies is expressed with reference to the "lines of inductive magnetic action;" and elementary proofs of the tendency of paramagnetic bodies toward places of stronger magnetic action, and of diamagnetic bodies toward places of weaker action, are given. This distinction of paramagnetic and diamagnetic is not here used absolutely, but indicates a greater or less conductivity for the lines of inductive action than that of the surrounding medium.

The magnetic phænomena of crystals are then examined, and referred to unequal magnetic conductivity in different directions; and the case of a crystalline sphere in a uniform field of force is worked out.

The laws of electric conduction, as laid down by Ohm, are shown to agree with those of the imaginary fluid, and definitions of quantity and intensity are given, which will apply to magnetism as well as galvanism.

The theory of the attractions of closed circuits, as established by Ampère, is shown to lead to the following results:—

1. The total intensity of the magnetizing force estimated along any closed curve embracing the circuit is a measure of the quantity of the current.

2. The quantity of the current, multiplied by the quantity of inductive magnetic action, from whatever source, which passes through it, gives what may be called the potential of the circuit. The tendency of the resultant forces is to increase this potential.

The theory of Faraday with respect to the induction of currents in closed circuits takes the following form :—

When the quantity of inductive magnetic action which passes through a given circuit changes in any way, an electromotive force proportional to the rate of change acts in the circuit, and a current is produced whose quantity is the electromotive force divided by the total resistance of the circuit.

The mathematical discussion of the electro-magnetic laws is reserved for another communication.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

February 11, 1856.

A paper was read by Mr. Maxwell of Trinity College, "On Faraday's Lines of Force," in continuation of a former paper (Proceedings of the Society, Dec. 10, 1855).

This paper was chiefly occupied with the extension of the method of lines of force to the phænomena of electro-magnetism, by means of a mathematical method founded on Faraday's idea of an "electrotonic state."

In order to obtain a clear view of the phænomena to be explained, we must begin with some general definitions of *quantity* and *intensity* as applicable to electric currents and to magnetic induction. It was shown in the first part of this paper, that electrical and magnetic phænomena present a mathematical analogy to the case of a fluid whose steady motion is affected by certain moving forces and resistances. [The purely imaginary nature of this fluid has been already insisted on.] Now the amount of fluid passing through any area in unit of time measures the *quantity* of action over this area; and the moving forces which act on any element in order to overcome the resistance, represent the total *intensity* of action within the element.

In electric currents, the *quantity* of the current in any given direction is measured by the quantity of electricity which crosses a unit area perpendicularly to this direction; and the intensity, by the resolved part of the whole electromotive forces acting in that direction. In a closed circuit, whose length $=l$, coefficient of resistance $=k$, and section $=C^2$, if F be the whole electromotive force round the circuit, and I the whole quantity of the current,

$$\frac{I}{C^2}\,lk=F, \quad I=\frac{C^2F}{lk}.$$

The laws of Ohm with respect to electric currents were then applied to cases in which the conducting power of the medium is different in different directions. The general equations were given and several cases solved.

In magnetic phænomena, the distinction of quantity and intensity is less obvious, though equally necessary. It is found, that what Faraday calls the quantity of inductive magnetic action over any

surface, depends only on the *number* of lines of magnetic force which pass through it, and that the total electromotive effect on a conducting wire will always be the same, provided it moves across the same system of lines, in whatever manner it does so. But though the quantity of magnetic action over a given area depends only on the number of lines which cross it, the intensity depends on the force required to keep up the magnetism at that part of the medium; and this will be measured by the product of the quantity of magnetization, multiplied by the coefficient of resistance to magnetic induction in that direction.

The equations which connect magnetic quantity and intensity are similar in form to those which were given for electric currents, and from them the laws of diamagnetic and magnecrystallic induction may be deduced and reduced to calculation.

We have next to consider the mutual action of magnets and electric currents. It follows from the laws of Ampère, that when a magnetic pole is in presence of a closed electric circuit, their mutual action will be the same as if a magnetized shell of given intensity had been in the place of the circuit and been bounded by it. From this it may be proved, that (1) the *potential* of a magnetized body on an electric circuit is measured by the *number* of lines of magnetic force due to the magnet which pass through the circuit. (2) That the total amount of work done on a unit magnetic pole during its passage round a closed curve embracing the circuit depends only on the *quantity* of the current, and not on the form of the path of the pole, or the nature or form of the conducting wire.

The first of these laws enables us to find the forces acting on an electric circuit in the magnetic field. Give the circuit any displacement, either of translation, rotation, or disfigurement, then the difference of potential before and after displacement will represent the force urging the conductor in the direction of displacement. The force acting on any element of a conductor will be perpendicular to the plane of the current and the lines of magnetic force, and will be measured by the product of the quantities of electric and magnetic action into the sine of the angle between the direction of the electric and magnetic lines of force.

The second law enables us to determine the quantity and direction of the electric currents in any given magnetic field; for, in order to discover the quantity of electricity flowing through any closed curve, we have only to estimate the work done on a magnetic pole in passing round it. This leads to the following relations between $\alpha_1\ \beta_1\ \gamma_1$, the components of magnetic intensity, and $a_2\ b_2\ c_2$, the resolved parts of the electric current at any point,

$$a_2=\frac{d\beta_1}{dz}-\frac{d\gamma_1}{dy},\quad b_2=\frac{d\gamma_1}{dx}-\frac{d\alpha_1}{dz},\quad c_2=\frac{d\alpha_1}{dy}-\frac{d\beta_1}{dx}.$$

In this way the electric currents, if any exist, may be found when we know the magnetic state of the field. When $\alpha_1 dx+\beta_1 dy+\gamma_1 dz$ is a perfect differential, there will be no electric currents.

Since it is the *intensity* of the magnetic action which is immedi-

ately connected with the *quantity* of electric currents, it follows that the presence of paramagnetic bodies, like iron, will, by diminishing the total resistance to magnetic induction while the total intensity is constant, increase its quantity. Hence the increase of external effect due to the introduction of a core of soft iron into an electric helix.

From the researches of Faraday into the induction of electric currents by changes in the magnetic field, it appears that a conductor, in cutting the lines of magnetic force, experiences an electromotive force, tending to produce a current perpendicular to the lines of motion and of magnetic force, and depending on the number of lines cut by the conductor in its motion.

It follows that the total electromotive force in a closed circuit is measured by the *rate of change* of the number of lines of magnetic force which pass through it; and it is indifferent whether this change arises from a motion of this circuit, or from any change in the magnetic field itself, due to changes of intensity or position of magnets or electric currents.

This law, though it is sufficiently simple and general to render intelligible all the phænomena of induction in closed circuits, contains the somewhat artificial conception of the number of lines *passing through* the circuit, exerting a physical influence on it. It would be better if we could avoid, in the enunciation of the law, making the electromotive force in a conductor depend upon lines of force external to the conductor. Now the expressions which we obtained for the connexion between magnetism and electric currents supply us with the means of making the law of induced currents depend on the state of the conductor itself.

We have seen that from certain expressions for magnetic intensity we could deduce those for the quantity of currents, so that the currents which pass through a given closed curve may be measured by the total magnetic intensity round that curve. Here we have an integration *round the curve itself* instead of one *over the enclosed surface*. In the same way, if we assume the mathematical existence of a state, bearing the same relation to magnetic quantity that magnetic intensity bears to electric quantity, we shall have an expression for the quantity of magnetic induction passing through a closed circuit in terms of quantities depending on the circuit itself, and not on the enclosed space.

Let us therefore assume three functions of $x\,y\,z$, $\alpha_0\,\beta_0\,\gamma_0$, such that $a_1\,b_1\,c_1$ being the resolved parts of magnetic quantity,

$$a_1=\frac{d\beta_0}{dz}-\frac{d\gamma_0}{dy},\quad b_1=\frac{d\gamma_0}{dx}-\frac{d\alpha_0}{dz},\quad c_1=\frac{d\alpha_0}{dy}-\frac{d\beta_0}{dx};$$

then it will appear that if we assume $\frac{d\alpha_0}{dt}$, $\frac{d\beta_0}{dt}$, $\frac{d\gamma_0}{dt}$ as the expressions for the electromotive forces at any point in the conductor, the total electromotive force in any circuit will be the same as that expressed by Faraday's law. Now as we know nothing of these inductive effects except in closed circuits, these expressions, which are

true for closed currents, cannot be inconsistent with known phænomena, and may possibly be the symbolic representative of a real law of nature. Such a law was suspected by Faraday from the first, although, for want of direct experimental evidence, he abandoned his first conjecture of the existence of a new state or condition of matter. As, however, we have now shown that this state, as described by him (Exp. Res. (60.)), has at least a mathematical significance, we shall use it in mathematical investigations, and we shall call the three functions α_0, β_0, γ_0, the *electrotonic functions* (see Faraday's Exp. Res. 60. 231. 242. 1114. 1661. 1729. 3172. 3269.).

That these functions are otherwise important may be shown from the fact, that we can express the potential of any closed current by the integral

$$\int\left(a_2\,\alpha_0\frac{dx}{ds}+b_2\beta_0\frac{dy}{ds}+c_2\gamma_0\frac{dz}{ds}\right)ds,$$

and generally that of any system of currents in a conducting mass by the integral

$$\iiint(\alpha_0 a_2+\beta_0 b_2+\gamma_0 c_2)dx\,dy\,dz.$$

The method of employing these functions is exemplified in the case of a hollow conducting sphere revolving in a uniform magnetic field (see Faraday's Exp. Res. (160.)), and in that of a closed wire in the neighbourhood of another in which a variable current is kept up, and several general theorems relating to these functions are proved.

February 25, 1856.

A paper was read, "On a direct method of estimating Velocities, Accelerations, and all similar magnitudes with respect to Axes moveable in any manner in Space, with applications." By Mr. Hayward, of St. John's College.

The frequent recurrence, in many different investigations of kinematics and dynamics, of exactly corresponding equations, suggests the inquiry whether they do not result from some common principle, from which they may be deduced once for all. An investigation based on this idea forms the first part of this paper, and the result is the method mentioned in the title.

This calculus shows how the variations of any magnitude, capable of representation by a straight line of definite length in a definite direction, and subject to the *parallelogrammic* law of combination, may be *simply* and *directly* determined relatively to any axes whatever. If such a magnitude (u) be estimated in a given direction, its intensity in that direction will be represented by the projection on it of the line which represents u. If this given direction be not fixed, but move according to a given law, the projection of u upon it will change by the alteration of its inclination to the direction of u; and the rate of that change is easily calculated, whence an expres-

sion for the acceleration of the resolved part of u along a given axis as *due to the motion of that axis*. If u itself be variable, its variations may be conceived to be due to an acceleration f in a definite direction, which in the time dt produces a quantity fdt in the direction of f to be combined with u by the parallelogrammic law; hence result expressions for the changes in intensity and direction of u. If, u being variable, the variations in its intensity estimated along a given moveable direction be sought, it will consist of two parts: one, that due to the resolved part of f in the given direction; the other, that due to the motion of the axis, which is the same as if f had not existed, or u had been constant: hence expressions for the total acceleration of the resolved part of u along the given moveable axis. If u be resolved along three rectangular axes, these expressions take the forms of familiar kinematical and dynamical equations.

These results furnish immediately expressions for the relative velocities of a point with respect to moving axes when its absolute velocities in their directions are given, and *vice versâ*. They also furnish very ready means of estimating accelerations in variable directions; as, for instance, the radial and transversal accelerations of a point moving in a plane or in space, or the tangential and normal accelerations in the same case. These are some kinematical applications of the calculus.

The dynamical applications form the second part of the paper. Here the general problem of the motion of a system, so far as it is due to *external* forces, is divided into two steps: one from *force* to *momentum*, the other from *momentum* to *velocity*. If the momenta of the particles of a system be reduced like a system of forces, they produce a single *linear momentum* and a single *angular momentum*, just as a system of forces produces a single force and a single couple. The linear momentum is (in our received language) the momentum of the mass of the system collected at its centre of gravity; the angular momentum is a magnitude the constancy of whose intensity in a given axis is equivalent to the assertion of the principle of the conservation of areas for that axis, and the constancy of whose directions determines the "invariable plane" as a plane perpendicular to it. The momentum, whether linear or angular, is a magnitude to which the previous calculus applies, and the resultant force and resultant couple are respectively the accelerators of the two kinds of momentum: hence the equations obtained in the first part, interpreted with respect to these magnitudes, furnish equations in any required form for the determination of the momenta at any instant. The step from force to momentum is independent of the nature of the system, that from momentum to velocity requires the system to be particularized. In the paper the case of an invariable system only is considered, and in particular its motion of rotation about its centre of gravity. The axis of rotation or angular velocity is related in direction to that of angular momentum, as the radius of the central ellipsoid with which it coincides to the normal at its extremity. Hence an angular momentum constant in intensity and direction, in general gives rise to an angular velocity variable in both respects,

and *vice versâ*. The question then becomes, to determine the acceleration of angular velocity due to the motion of the system. This is obtained by determining the acceleration of angular momentum for a line fixed in the body, which is then shown to be a maximum for the normal to the plane containing the axes of angular momentum and velocity; then the acceleration along this line is the total acceleration of angular momentum due to the motion, and the acceleration of angular velocity determined from it (just as the angular velocity is determined from the momentum) is that due to the motion of the system. Also the acceleration of angular velocity due to the forces is related to the resultant couple and its axis, just as the angular velocity to the angular momentum. Thus the accelerations of angular velocity due both to the motion and to the forces being determined, the intensity and direction of the angular velocity at any time is to be found by combining these effects by integration. The problem is worked out in the case of the axis of the resultant couple being coincident with that of angular momentum, so that this remains fixed. The paper concludes with a simple solution of the problems of Foucault's gyroscope as applied to show the effects of the earth's rotation, the simplicity arising from the method of this paper enabling us at once to refer the motion to those axes (neither fixed in the body nor in space) whose motion it is desired to determine.

March 10, 1856.

A paper was read by L. Barrett, Esq., "On the Distribution of the Mollusca on the Coast of Norway." (*Vide* Annals of Nat. Hist. May 1856.)

In this the author observed, that when the fauna of the coast of Norway is compared with that of the other side of the North Atlantic, a great difference will at once be perceived, not only in the number of species, but also in the different distribution of northern and southern types; the Mollusca of Greenland being peculiarly arctic, those of Scandinavia a mixture of southern and northern species. In the southern part of Norway we find the species living on our coasts abundant; but they become rarer as we go north, their place being supplied by arctic forms. Many of the northern species have a great geographical range, at which we need not be surprised when we consider their great antiquity, many of them having existed since the pliocene period; and, in the author's opinion, whenever we find a species with a great geographical range, we may at once infer that it has continued to live from a remote period. It is extremely difficult, according to the present state of the currents in the northern seas, to account for the wide distribution of arctic shells on this side of the North Atlantic; but when we consider that at not a very distant period the temperature and other conditions of this area were totally different, that a cold climate prevailed, certainly accompanied by a current setting from the north (as is fully proved by the

fact that boulders are always found nearly south of the mountain ranges from which they have been originally transported), and that many of the shells are found fossil in the Sicilian tertiaries, this wide distribution may be fully accounted for. As these frigid conditions were gradually altering to more genial ones, those species requiring a lower temperature would gradually die out, and only continue to exist in higher latitudes. The littoral and shallow water species would be most affected by such an alteration of climate; and while the fauna of the littoral and laminarian zones would be entirely changed,—the shells composing that fauna replaced by other species,—those living in the deep sea would continue to exist, perhaps at a greater depth, mingled with the species brought in with the new physical conditions of the area. This we know to be the case; for while the northern littoral shells, such as *Mya truncata*, &c., are found only fossil in Sicily, many of the deep-sea arctic species that existed there when those fossils were alive are still found living in the deeper parts of the Mediterranean.

The same thing occurs on our coasts, where the arctic littoral or shallow-water shells, as *Astarte arctica, Tellina proxima, Natica helicoides*, &c., which are found in shallow water on the Scandinavian or Greenland coasts, are now rare as deep-sea shells, and that in the same area in which they were formerly abundant as shallow-water species. Some species are capable of enduring great differences of climate, the *Mytilus edulis* being found as abundantly on the coast of Greenland as on our own shores.

It is not difficult to account for the presence of the southern species on the coast of Norway, as the Gulf-stream sets directly along the coast, warming its waters, and rendering them habitable for species requiring a moderately high temperature. The great abundance and wide distribution of these species show that the present order of things has continued for a great length of time. The gradual extinction of northern shells on our coasts is still going on; the number of living specimens of *Pecten danicus* is very small, while dead shells are very abundant, and fresh dead specimens of *Pecten islandicus* are frequently dredged, though a living specimen has not yet occurred. It is probable that this species has died out very recently.

On the eastern shores of Davis's Straits the Mollusca are about half as numerous as on the coast of Norway. The fauna differs in the prevalence of arctic types and the total absence of southern. At a former period the fauna was of a mixed character; species now inhabiting more southern latitudes are found fossil in the raised beaches at Disco Island, which species are no longer found living on the coasts.

April 28, 1856.

A paper was read "On the Theory of Heat," by Mr. A. A. Harrison, of Trinity College.

The object of this paper was to show that there is considerable

reason for supposing that radiant heat is identical with light, and that they both consist of vibrations of the ultimate particles of matter.

There is a strong presumption of this from the facts, that every body heated to a certain temperature, dependent only on the nature of the surface, emits light as well as heat; and that "whenever light manifests itself, heat appears along with it" (Kelland): the difference between radiant heat and ordinary heat is, that radiant heat is due to vibrations in planes normal to its direction of propagation, and that ordinary heat consists of vibrations in all three dimensions.

The author endeavoured to show, in the first place, that the motions of the particles of matter, which must be caused by friction, or in the union of two gases in combustion, is sufficient of itself to account for the following phænomena of heat:—

I. That a body once heated continues of the same temperature, with the exception of heat lost by radiation, conduction, &c. This follows immediately from the principle, that in any system of particles held together by mutual attractions and repulsions, the *vis viva* is independent of the time, and depends merely on the position of the particles.

II. That bodies expand by heat.

Before proceeding to this, the author argued that in gases the repulsive force varies inversely as the cube, and not, as usually stated, the simple power of the distance; that it is not true, without some limitation, that the force varies as the inverse first power, was urged from the fact that such a force would decrease more slowly than one varying as the inverse square, and consequently would be the force observed in astronomical phænomena; and even the oxygen of the ocean would repel that of the air instead of attracting it. That the force varies as the inverse cube was deduced from the law of elasticity, that the density varies as the pressure; for if a particle repels other particles with a force varying as the inverse cube, it repels a fixed plane of them with a force varying as the inverse first power. That this is the case may be seen, by considering that though the particle repels particles similarly situated with a force varying as the inverse cube, yet the number of such particles varies directly as the square of the distance, and therefore the whole effect upon the plane varies inversely as the first power. And if this is true for a plane, it is also true for the solid side of the containing vessel; for any solid may be considered as made up of a succession of planes.

The law being the inverse cube, it follows that in any position the sum of the forces exerted by any particle on two particles, one on each side of it, is least when that particle is half-way between them, and increases the further the particle is removed from the middle point. This is seen directly, for the value of $\frac{\mu}{(a+x)^3}+\frac{\mu}{(a-x)^3}$ is least when $x=0$, and increases until $x=a$. And therefore, in order to produce the same force, it would be necessary that the mean distance should be increased; and hence if the particles of any aëriform body be in motion, the force exerted by them would be greater than when at rest; that is, if the pressure to be supported be con-

stant, the average distance of the particles must increase, and the body must expand.

III. That every aëriform body not in contact with a liquid expands in the same proportion. This was accounted for by the circumstance, that the increase of pressure depends only upon the ratio of the disturbance to the original distance, and not at all upon the absolute distance.

IV. That air and elastic fluids give out heat on compression. By compression the absolute distance of the particles from one another is diminished; but the absolute motion remaining the same, the relative motion is increased.

V. That the same amount of heat is generated in two gases subjected to the same pressure; for the absolute distance of the particle in both being diminished in the same proportion, and the absolute motion remaining unaltered, the relative motion is increased in the same proportion in both.

VI. The specific heats are inversely as the atomic weights. Here it was necessary to show that mass is not necessarily proportional to the quantity of matter, as usually stated; or rather, that a body may have a different mass when considered with regard to the molecular force from what it has with respect to the force of gravity. With regard to elasticity of gases, the weight of any single particle is so small as not to affect the result. The question remains, whether we know anything of the masses of different particles relatively to this repulsive force. To determine their masses we have these data. In several different gases equivalent volumes under the same pressure occupy the same space, that is (assuming the Daltonian theory, that equivalent volumes contain the same number of particles), that each particle of the two different gases exerts an equal pressure on the adjacent particles: and hence, with reference to this law, the mass of a single particle in each of these two different gases is the same, and therefore the "*vis viva*" of equivalent weight or volumes subject to the same motion is the same for both; that is, the quantity of heat of an equivalent of each is the same, and therefore the specific heat of a given weight is inversely as its equivalent number or atomic weight.

With reference to the phænomena of radiation, it may be shown from theoretical considerations that the inverse cube is the law required. The inverse first is impossible, for then there could be no vibrations. For the same reason the inverse second is impossible (Camb. Phil. Trans. vol. vii. p. 98). The inverse fourth is also impossible, for then there could be no vibrations, and the velocity would be infinite (vol. vi. p. 325). It has also been shown that neither the second nor the fourth would satisfy the conditions of the equations (vol. vii. p. 419). Hence, from the theory of radiation, it is supposed that the luminiferous æther consists of solid particles, attracting one another with a force varying as the inverse square (vol. vii. p. 110), and repelling with a force varying as the inverse cube.

Now from the Daltonian theory, and the law of elastic fluids, it

has been shown that the ultimate particles of our atmosphere compose such an æther. But if our atmosphere is the luminiferous æther, we must next inquire whether it does pervade space. Omitting variations of temperature, and merely considering the atmosphere as subject to the two forces of elasticity and gravity, we have for the equation of a column of air on a unit of surface,

$$dp = -\frac{ga^2}{z^2}\rho dz, \text{ or } \frac{1}{p}\frac{dp}{dz} = \frac{kga^2}{z^2}, \text{ where } k = \frac{\rho}{p}.$$

Integrating this, we find that p and ρ, though they become extremely small, never vanish; and therefore, if these laws are absolutely true, our atmosphere does pervade space.

It may be well to obviate the objection, that black substances radiate heat best, and white substances light. This arises from employing the same word radiation to denote two different things: by radiated heat is meant heat given out from a heated body; by radiated light is meant the secondary radiation from the surface of a body exposed to light.

If Sir J. Leslie's experimental calculation of the heat lost from the sun be correct, there is no need of any theories to account for its generation.

From the foregoing arguments and facts, it was urged that motions and forces, which certainly exist in cases of combustion, would produce phænomena exactly similar to those of heat, and therefore that part of the phænomena usually attributed to heat are due to this motion; and if part of them, probably the whole. And further, that if the phænomena of radiation of heat are explained by this motion of the particles of matter, light is simply radiated heat of considerable intensity; and that imponderable substances, whether under the names of æther, caloric, or phlogiston, are equally imaginary.

Also, a paper was read "On the Question—What is the Solution of a Differential Equation?" By Professor De Morgan.

This paper is a short supplement to § 3 of a paper on some points of the integral calculus (Camb. Trans. vol. ix. part 2). It discusses the principles on which such an equation as $y'^2 = a^2$, giving

$$(y - ax + b)(y + ax + c) = 0,$$

is generally affirmed to be completely solved when $b = c$. It dwells on the distinction between a *relation* and an *equation*, which may express the alternative of one or more relations; it points out several cases in which conclusions applicable to the simple relation only are affirmed of any equation; and, with reference to the question asked in the title, discusses the manner in which the answer depends on the cross-question, what degree of discontinuity is allowed to be implied in the word *solution*?

May 12, 1856.

A paper was read by Mr. Warburton "On Self-repeating Series," in continuation of a former paper.

The author showed in his former paper on self-repeating series, printed in vol. ix. part 4. of the 'Transactions' of the Society, that in the fraction which generates a series of either of the following forms,

$$1^{2n} \pm 2^{2n} . t + 3^{2n} . t^2 \pm \&c. \ldots,$$

or

$$1^{2n+1} \pm 2^{2n+1} . t + 3^{2n+1} t^2 \pm \&c. \ldots,$$

the numerator of such fraction is a recurrent function of t. He also then determined the coefficients of the several powers of t in such numerator to be given linear functions of the differences (as the case may be) of 0^{2n}, or of 0^{2n+1}.

In his present paper, from the n pairs of equal coefficients which the recurrent numerator contains, the author obtains n linear equations between the $2n$ differences concerned; and selecting any n of these differences, he concludes that each of them can be expressed in terms of the other n differences not so selected; and consequently that no formula, expressed in terms of the differences of 0^{2n} or 0^{2n+1}, need contain more than n of those differences.

He gives the equations requisite for obtaining $\Delta^{n+p}(0^{2n})$ in terms of $(\Delta^n, \Delta^{n-1}, \ldots \Delta^2, \Delta^1)0^2$; and $\Delta^{n+1+p}(0^{2n+1})$, in terms of $(\Delta^{n+1}, \Delta^n, \ldots \Delta^3, \Delta^2)0^{2n-1}$; and he applies these and other of his equations to the elimination of particular differences of zero from sundry formulas.

Also, Mr. Bashforth exhibited models illustrating the Moon's motion.

Also, a paper was read by Mr. Maxwell "On the Elementary Theory of Optical Instruments."

The object of this communication was to show how the magnitude and position of the image of any object seen through an optical instrument could be ascertained without knowing the construction of the instrument, by means of data derived from two experiments on the instrument. Optical questions are generally treated of with respect to the pencils of rays which pass through the instrument. A pencil is a collection of rays which have passed through one point, and may again do so, by some optical contrivance. Now if we suppose all the points of a plane luminous, each will give out a pencil of rays, and that collection of pencils which passes through the instrument may be treated as a *beam* of light. In a pencil only one ray passes through any point of space, unless that point be the focus. in a beam, an infinite number of rays, corresponding each to some point in the luminous plane, passes through any point; and we may,

if we choose, treat this collection of rays as a pencil proceeding from that point. Hence the same beam of light may be decomposed into pencils in an infinite variety of ways; and yet, since we regard it as the same collection of rays, we may study its properties as a beam independently of the particular way in which we conceive it analysed into pencils.

Now in any instrument the incident and emergent beams are composed of the same light, and therefore every ray in the incident beam has a corresponding ray in the emergent beam. We do not know their path within the instrument, but before incidence and after emergence they are straight lines, and therefore any two points serve to determine the direction of each.

Let us suppose the instrument such that it forms an accurate image of a plane object in a given position. Then every ray which passes through a given point of the object before incidence passes through the corresponding point of the image after emergence, and this determines one point of the emergent ray. If at any other distance from the instrument a plane object has an accurate image, then there will be two other corresponding points given in the incident and emergent rays. Hence if we know the points in which an incident ray meets the planes of the two objects, we may find the incident ray by joining the points of the two images corresponding to them.

It was then shown, that if the image of a plane object be distinct, flat, and similar to the object for two different distances of the object, the image of any other plane object perpendicular to the axis will be distinct, flat, and similar to the object.

When the object is at an infinite distance, the plane of its image is the *principal focal plane*, and the point where it cuts the axis is the *principal focus*. The line joining any point in the object to the corresponding point of the image cuts the axis at a fixed point called the *focal centre*. The distance of the principal focus from the focal centre is called the *principal focal length*, or simply the *focal length*.

There are two principal foci, &c. formed by incident parallel rays passing in opposite directions through the instrument. If we suppose light always to pass in the same direction through the instrument, then the focus of incident rays when the emergent rays are parallel is the *first* principal focus, and the focus of emergent rays when the incident rays are parallel is the *second* principal focus. Corresponding to these we have first and second focal centres and focal lengths.

Now let Q_1 be the focus of incident rays, P_1 the foot of the perpendicular from Q_1 on the axis, Q_2 the focus of emergent rays, P_2 the foot of the corresponding perpendicular, F_1F_2 the first and second principal foci, A_1A_2 the first and second focal centres, then

$$\frac{P_1F_1}{A_1F_1}=\frac{P_1Q_1}{P_2Q_2}=\frac{F_2P_2}{F_2A_2},$$

lines being positive when measured in the direction of the light.

Therefore the position and magnitude of the image of any object is found by a simple proportion.

In one important class of instruments there are no principal foci or focal centres. A telescope in which parallel rays emerge parallel is an instance. In such instruments, if m be the angular magnifying power, the linear dimensions of the image are $\frac{1}{m}$ of the object, and the distance of the image of the object from the image of the object-glass is $\frac{1}{m^2}$ of the distance of the object from the object-glass. Rules were then laid down for the composition of instruments, and suggestions for the adaptation of this method to second approximations, and the method itself was considered with reference to the labours of Cotes, Smith, Euler, Lagrange, and Gauss on the same subject.

November 6, 1856.

A paper was read by Dr. Donaldson "On the Structure of the Athenian Trireme, considered with reference to certain difficulties of Interpretation."

The author's intention was to show in this paper that the arrangements for seating the three tiers of rowers in the trireme, which Dr. Arnold has called "an indiscoverable problem," may be adequately explained by an examination of the terms which are used to discriminate the rowers, and of other words referring to the different parts of the war-galley. The name of the *zygitæ*, or rowers of the middle tier, implies that they sat on the ζυγά, or transverse planks connecting the opposite sides of the vessel, also called σέλματα, and in earlier times κληῖδες. The *thalamitæ*, or rowers of the lowest tier, must, in accordance with their name, have had their seats attached to the ribs of the vessel in the θάλαμος, or hold. And the *thranitæ*, or rowers of the highest tier, sat on θρῆνες, or benches like low stools, extending for seven feet along the alternate ζυγά. The *epibatæ*, or marines, whether as working the supernumerary oars, or as fighting, occupied platforms running along the bulwarks. This view of the matter explained the fact that there was a gangway from the stern to the prow for the passage of the officers, &c. along the σέλματα or ζυγά, between the ends of the stools on which the thranitæ sat. This gangway was called the σελίς, and the same name was given to the passages leading down to the orchestra from the upper part of the theatre between the rows of seats occupied by the spectators. Hence was derived a philological explanation of the words in Aristoph. *Equites*, 546:—

> αἵρεσθ' αὐτῷ πολὺ τὸ ῥόθιον, παραπέμψατ' ἐφ' ἕνδεκα κώπαις
> θόρυβον χρηστὸν ληναΐτην·

for there were eleven tiers of seats between each *diazoma* of the theatre, which were divided again by the *selis*; so that the spectators

would represent eleven banks of oars, seated, as in the trireme, with the lower rows in advance. In the same way, the use of the *selis* in a trireme, as the gangway for the officers, &c., explains the lines in the *Agamemnon* of Æschylus, 1588–9 :—

> σὺ ταῦτα φωνεῖς νερτέρᾳ προσήμενος
> κώπῃ, κρατούντων τῶν ἐπὶ ζυγῷ δορός ;

for if the *zygitæ* had been intended, they must have been described as τῶν ἐπὶ ζυγῶν. The same view of the σέλματα, as the proper place for the officers, was used to explain another passage in the same play (v. 1413), where Agamemnon's companion is described as ναυτικῶν σελμάτων ἱστοτριβής. And the risk of passing along these planks, with intervals between them, was considered to explain the proverbial warning that we must take care not to miss our footing and fall into the hold (Eurip. *Heracl.* 168). Other points were incidentally noticed.

November 10, 1856.

The Master of Trinity read a paper "On the Platonic Theory of Ideas."

In this, he first stated the Platonic theory of ideas as given by the late Professor Butler of Dublin, in his 'Lectures' (vol. ii. p. 117); he then remarked that this theory had evidently, for one of its objects, to explain the possibility of necessary, and therefore eternal truths; and thus was an attempt to solve the problem, often debated in modern times, of the grounds of mathematical truth; an attempt especially called out by the Heraclitean skepticism of Plato's time. The doctrine of ideas which belong to the *intelligible*, not to the visible *world*, and which are the basis of demonstration, did really answer its purpose, and account for the existence of real and eternal truths; and at the same time, by the tenet that sensible things participate in those ideas, accounted for the securing of truth respecting the sensible world. But when Plato goes on to speak of ideas of tables and chairs and the like, he gives an extension of the theory which solves no difficulty, and for which no valid reason is rendered.

The arguments against this extension of the theory are given with great force in the Dialogue entitled *Parmenides*, and are not answered there, nor in a satisfactory way, in any part of Plato's writings. Moreover, throughout this Dialogue, Parmenides is represented as having, in his conversation with Socrates, vastly the superiority, not only in argument, but in temper and manner; and Socrates and his friends, after a little show of resistance, assent submissively to all that Parmenides says. On this ground the writer maintains that the Dialogue is not Plato's, but anti-Platonic, written probably by an admirer of Parmenides, and tending to represent Socrates and his disciples as poor philosophers, conceited talkers, and feeble disputants.

This view was further confirmed by arguments drawn from the external circumstances of the Dialogue.

November 24, 1856.

An account was given by Professor Miller, of the restoration of the Standard of Weight (*vide* Proceedings of Royal Society, vol. viii. Nos. 21, 22).

December 8, 1856.

Mr. Humphry read a paper "On the relations of the Vertebrate Skeleton to the Nervous System."

He pointed out that the central parts, both of the skeleton and of the nervous system, are composed of segments placed in front of, or above one another: those of the former being called "vertebræ," those of the latter "ganglia;" that the vertebræ correspond with the ganglia, each vertebra having its appropriate ganglion; and further, that the processes, or nerves, emanating from the central ganglionic portion of the nervous system correspond with, and accompany the processes, or bones, appended to the central portion of the skeleton, so that the bones appended to any particular vertebra are generally accompanied by the nerves emanating from the ganglion connected with that vertebra. Hence, where a difficulty is found in referring a bone to its vertebra, assistance may often be derived from a reference to the nerve or nerves which accompany that bone. Following the guide thus indicated, Mr. Humphry would refer the upper extremity, not to one—the occipital—vertebra, according to the plan of Professor Owen, but to several cervical vertebræ, forasmuch as it derives its nerves from a considerable tract of the cervical portion of the cord. For the like reason, the lower extremity may be regarded as appertaining to several lumbar and sacral vertebræ. The relations of the bones of the face to their respective cranial vertebræ were pointed out in accordance with the distribution of the cerebral nerves. It was shown, that although the size and shape of the skull are proportioned to the size and shape of the brain, yet that, as a general rule, the thickness and weight of the skull are in an inverse ratio to the size of the brain. A comparison of the different nations of mankind proves, moreover, that the size of the whole skeleton, as well as that of the skull, is usually proportioned to the size of the brain; a well-developed *physique* being the natural associate of an ample *cerebrum*.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

February 9, 1857.

Prof. Challis gave an account of his Observation of the Occultation of Jupiter on Jan. 2, 1857.

February 23, 1857.

A paper was read "On the Theory of Polarized Fasciculi, commonly known as Haidinger's Brushes." By the Rev. J. Power, M.A., Librarian of the University.

In this paper the view taken of the subject is similar to that which had occurred to M. Jamin, and which will be found in Poggendorff's *Annalen*, 1849, p. 145, and in the *Comptes Rendus*, tome xxvi. p. 197. The author arrived, however, at the present theory quite independently in the course of last summer, and before he had acquainted himself with the literature of the subject. M. Jamin had taken as an *essai de calcul* the particular semi-visual angle 20°, which lies far beyond the limits within which the phænomenon is visible; and he has not attempted to give the general law for small angles, which was the real problem to be solved.

This is what the author has attempted in the present communication, availing himself of the experimental researches of Chossat given in the *Bulletin de la Soc. Philomatique*, 1818, p. 94.

The subject was rendered more complicated by the circumstance that the formulæ for the intensities of the refracted pencils are given differently by Neumann, Airy, and the author of this paper. Instead of taking any one set of formulæ, the author managed to take them all into consideration by previously showing that Airy's formulæ result from Neumann's by multiplying them by $\frac{\tan\theta_{\prime}}{\tan\theta}$, which is equivalent to $\frac{1}{\mu}\cdot\frac{\cos\theta}{\cos\theta_{\prime}}$; while his own result from the same by multi-

plying them by $\frac{\cos\theta}{\cos\theta_{\prime}}$; θ and $\theta_{\prime}$ being the angles of incidence and refraction.

It follows from thence that, whichever set of formulæ we may prefer, we shall have after refraction, in all cases, the following simple relation for two oppositely polarized incident pencils of equal intensity; namely,

$$\frac{\text{intensity of pencil polarized in the plane of incidence}}{\text{intensity of the pencil polarized at right angles}}$$

$=\cos^2(\theta-\theta_{\prime})$.

This fraction decreases as the deviation $\theta-\theta_{\prime}$ increases; it is therefore less for the violet rays than the red, for the indigo than for the yellow; and this serves to explain in a general way the dingy yellow stripe in the plane of polarization, and the bright violet stripe in the plane at right angles.

The author has also considered the effect in a plane making an angle ϕ with the plane of greatest polarization, and arrives at the following result: that, provided we attribute to the distribution of the optic nerve such a variation of sensibility as, taken in conjunction with the action of the iris, shall produce a field of view uniformly bright from the centre outwards when common daylight is viewed (a condition which the author believes is common to all eyes with his own), we shall have for the brightness at any point of the field of view the following expression,

$$M.(1-e\gamma\frac{\theta^2}{2}\cos 2\phi),$$

where M is the central brightness, e the degree of polarization (being 0 for common daylight, and 1 for completely polarized light), and $\gamma=\cdot 07309$ for rays of mean refrangibility.

The last expression gives us without difficulty the form of the curves of equal brightness.

Assuming this constant brightness to be c M, and putting

$$x=\theta\cos\phi,$$

$$y=\theta\sin\phi,$$

we find for the equation sought

$$x^2-y^2=\frac{2(1-c)}{e\gamma}$$

The curves are therefore equilateral hyperbolas having the lines in octants for their common asymptotes, which confound themselves with the curves themselves when $c=0$, the case of mean brightness.

The yellow fasciculi have their vertices in the plane of polarization, and the violet fasciculi have their vertices in the plane at right angles.

It will be seen that for a given value of θ, the brightness, for rays

of all degrees of refrangibility, that is for all values of γ, is least in the plane of greatest polarization and greatest in the plane at right angles—contrary to the idea of Moigno, who, for insufficient reasons, imagined that the maximum occurred in the plane of polarization and the minimum in the plane at right angles.

The yellow tint in the position of minimum intensity, and the violet tint in the position of maximum intensity, is nevertheless perfectly accounted for by the consideration that γ is greater for the violet and indigo rays than for the red and yellow.

The paper further contains some observations respecting a subjective centre of the eye, distinct from the usual objective centre, which may be read with interest, as they remove some difficulties connected with the theory of vision, which had often occurred to the author, and may have occurred to others.

March 9, 1857.

Mr. Hopkins gave an account of some experiments on the conductivity of various substances, and pointed out the bearing of the results on theories of terrestrial heat.

April 27, 1857.

Mr. Humphry read a paper "On the Proportions of the Human Frame."

May 11, 1857.

A paper was read by Professor Stokes, "On the Discontinuity of Arbitrary Constants which appear in Divergent Developments."

In a paper "On the Numerical Calculation of a class of Definite Integrals and Infinite Series" printed in the ninth volume of the 'Cambridge Philosophical Transactions,' the author succeeded in putting the integral $\int_0^\infty \cos\frac{\pi}{2}(w^3-mw)dw$ under a form which admits of receiving every numerical calculation when m is large, whether positive or negative. The integral is obtained in the first instance under the form of circular functions for m positive, or an exponential for m negative, multiplied by series according to descending powers of m. These series, which are at first convergent, though ultimately divergent, have arbitrary constants as coefficients, the determination of which is all that remains to complete the process. From the nature of the series, which are applicable only when m is large, or when it is an imaginary quantity with a large modulus, the passage from a large positive to a large negative value of m cannot be made

through zero, but only by making m imaginary and altering its amplitude by π. The author succeeded in determining directly the arbitrary constants for m positive, but not for m negative. It was found that if, in the analytical expression applicable in the case of m positive, $-m$ were written for m, the result would become correct on throwing away the part involving an exponential with a positive index. There was nothing however to show *à priori* that this process was legitimate, nor, if it were, at what value of the amplitude of m a change in the analytical expression ought to be made, although the occurrence of radicals in the descending and ultimately divergent series, which did not occur in ascending convergent series by which the function might always be expressed, showed that some change analogous to the change of sign of a radical ought to be made in passing through some values of the amplitude of the variable m. The method which the author applied to this function is of very general application, but is subject throughout to the same difficulty.

In the present paper the author has resumed the subject, and has pointed out the character by which the liability to discontinuity in the arbitrary constants may be ascertained, which consists in this, that the terms of an associated divergent series come to be regularly positive. It is thus found that, notwithstanding the discontinuity, the complete integrals, by means of divergent series, of the differential equations which the functions treated of satisfy, are expressed in such a manner as to involve only as many unknown constants as correspond to the degree of the equation.

Divergent series are usually divided into two classes, according as the terms are regularly positive, or alternately positive and negative. But according to the view here taken, series of the former kind appear as singularities of the general case of divergent series proceeding according to powers of an imaginary variable, as indeterminate forms in passing through which a discontinuity of analytical expression takes place, analogous to a change of sign of a radical.

A communication was likewise made by the Rev. W. T. Kingsley, "On the application of Photography to Wood Engraving."

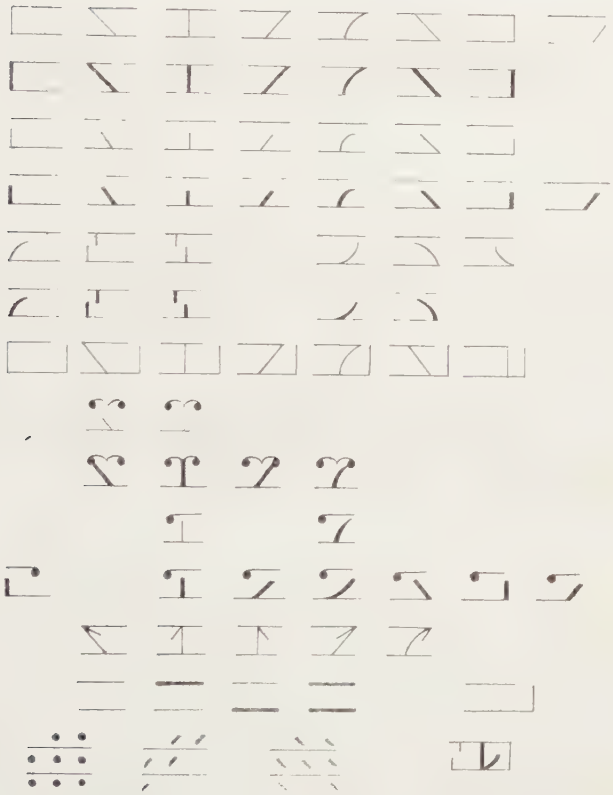
May 25, 1857.

Mr. Bashforth made a communication "On some Calculations and Experiments undertaken for the purpose of testing the Theories of Capillary Action."

Also Mr. Candy exhibited a Physiological Alphabet.

The principle of this alphabet is to make the form of the letter indicate the manner in which the sound is produced, by showing the position of the organs of speech concerned.

CANDY'S PHYSIOLOGICAL ALPHABET.



J. Palmer, Camb.

The open mouth, affording an uninterrupted, unimpeded passage to the breath, is represented by two horizontal straight lines ( ).

The sudden, complete, closing of the lips, which gives the sound of the letter P, is represented by drawing a line joining these two on the left ( ); the mouth looking in the same direction as the Queen's head on the coins and stamps.

All the letters in the first *column* of the Table have a line on the left, and are therefore labials.

The second column, having a line inclining to the left, are dentals.

The third column have a vertical line in the centre of the mouth, and are the linguals.

The fourth column incline to the right, and are the palato-linguals or weakened linguals.

The fifth column, with a line curving over to the right, are the Sanscrit cerebrals, in which the tongue is curled up, so that the lower side of it comes against the roof of the mouth.

The sixth column, in which the line leans from the right to the centre, are the palatals.

The seventh column, with a vertical line on the right, are the gutturals, at the back of the mouth.

The eighth column, with a line on the right, leaning to the right, are the faucals, still further back than the gutturals.

The first *row* of letters in the Table, with a thin line from top to bottom, are the mute explodents, in which there is a sudden and complete stoppage of the breath.

The second row, with a thick line, are the corresponding sonant explodents.

The third row, in which there is an interval between the top line and the connecting line, are the whispered continuants, in which there is a passage of breath, producing a hissing.

The fourth row, with a short thick line, are the corresponding voiced continuants, or buzzes.

The fifth row contains additional hissing letters, and the sixth row corresponding additional buzzing ones.

In the letters of the seventh row, there is represented an opening upwards at the back of the mouth, leading into the nose. These, therefore, are the nasal letters.

The double curl in the eighth and ninth rows indicates a vibration of both sides of the tongue, while the tip is fixed. This is the case in L, which is represented in Sanscrit by a similar form. The eighth row, with thin lines, are the whispered Ls.

The tenth and eleventh rows, with a single curl, are the Rs or trills. The curl indicates vibration.

The twelfth row are the Caffre clicks. The short line darting out from an angle, shows that the tongue is placed in a certain position, and then suddenly jerked away.

The thirteenth row are the breathings.

The dots and accents (· ′ ‵) in the last row are the vowel-points.

The last figure shows the applicability of this alphabet to monograms, being a combination of F, DZh=J, and K=C, the initials of the inventor, F. J. Candy.

The analysis of sound exhibited in this alphabet is that in 'Universal Writing and Printing,' by Mr. A. J. Ellis, a Fellow of this Society.

The following is Mr. Ellis's Universal Digraphic Alphabet arranged in the order of this alphabet, to facilitate comparison.

p	*tt*	*t*	*tj*	*ṭ*	*kj*	*k*	*q*
b	*dd*	*d*	*dj*	*ḍ*	*gj*	*g*	
ph	*ss*	*s*	*sj*	*sh̓*	*ch*	*kh*	
bh	*zz*	*z*	*zj*	*zh̓*	*jh*	*gh*	ع
wh	*f*	*th*		*sh*	*yh*	*x*	
w	*v*	*dh*		*zh*	*y*		
m	*nn*	*n*	*nj*	*ṇ*	*ngj*	*ng*	
	llh	*lh*					
	ll	*l*	*lj*	*ḷ*			
		rh		*ṛ*			
brh		*r*	*rj*	*zrh*	ɹ	R	*grh*
	cc	*c*	*ck*	*cj*	*c̓*		
	ʔ	;	H	H*h*		ʕ	

Key to the Consonants and Breathings.

[See the Table for the forms of the Physiological consonants.]

Row 1.
- *p* P, π, פ with dagesh.
- *tt* Arabic dental *t*, Hebrew ט.
- *t* T, τ, ת with dagesh.
- *tj* like *ty*.
- *ṭ* Sanscrit cerebral *t*.
- *kj* French *qu*, Italian *chi*.
- *k* K, κ, כ with dagesh.
- *q* Hebrew ק.

2.
- *b* B, ב with dagesh.
- *dd* Arabic dental *d*.
- *d* D, ד with dagesh.
- *dj* Hungarian *gy*.
- *ḍ* Sanscrit cerebral *d*.
- *gj* Old Engl. *gu*ard, French *gu*eux.
- *g* G, ג with dagesh.

3.
- *ph* φ, פ without dagesh.
- *ss* Arabic dental *s*, Hebrew צ.
- *s* S, σ, ס.
- *sj* Polish *ś*, Hebrew שׂ.
- *sh̓* Sanscrit cerebral *sh*.
- *ch* German palatal *ch* in "ich."
- *kh* German guttural *ch* in "ach!"

4. *bh* German *w*, ב without dagesh.
zz Arabic dental *z*.
z Z, ζ, ז.
zj Polish *ź*.
zh buzz of the hiss *sh*.
jh German palatal *g* in "teig."
gh German guttural *g* in "tag."
ε Hebrew ע, Arabic ع.

5. *wh* whispered *w*, English *wh*, Saxon *hw*.
f F : in which the lower lip touches the upper teeth.
th θ, ת without dagesh.
sh ש, English *sh*, French *ch*, German *sch*.
yh whispered *y* in "hue."
x Spanish *x* or *j*.

6. *w* English *w*, Hebrew ו.
v English *v*.
dh *th* in "the," ד without dagesh.
zh French *j*.
y English *y*, German *j*, Hebrew י.

7. *m* M, μ, מ.
nn Arabic dental *n*.
n N, ν, נ.
nj Fr. and Ital. *gn*, Span. *ñ*, Port. *nh*.
n Sanscrit cerebral *n*.
ngj back palatal *n*.
ng *ng* in "sing," γ before gutturals.

8. *llh* whispered Polish *l*.
lh whispered *l*, Welsh *ll*.

9. *ll* Polish barred *l*.
l L, λ, ל.
lj Ital. *gli*, Span. *ll*, Port. *lh*.
l Sanscrit cerebral *l*.

10. *rh* whispered *r*, Welsh *rh*.
r Sanscrit cerebral *r*.

11. *brh* German lip-trill.
r initial R, ρ, ר.
rj palatal *r*.
zrh Polish *rz*.
ɹ English *r* in "fir," "her."
ʀ English *r* in "fur," "poor."
grh Arabic غ.

12. { *cc* dental click, African *q*.
c flat click, African *c*.
ck side click, African *x*.
cj palatal click, African *qc*.
c̣ cerebral click.

— *ɿ*, Hebrew א, Greek ʼ.

— ; Arabic Hamza.

— H H ʿ ה.

— H*h* strong *h*, Hebrew ח.

— | *a* Sanscrit anuswara, mark of nasalization.

Vowels.

There are three *series* of vowels; the upper, middle, and lower; the points representing which are placed respectively above, between, and below the two horizontal lines of the letter.

There are also three *positions* of vowels; labial and dental (which may be called front), palatal, and guttural. The points are placed respectively at the left hand, middle, and right of the letter.

In some cases there are three vowels of the same series in the same position. These are expressed respectively by · ˊ ˋ in the appropriate place.

The number of vowels which may be expressed is thus twenty-seven; but there are only twenty distinct vowels recognized, of which ten occur in English. There are no front upper vowels.

Each of these twenty vowels may be long or short. The long vowels may be expressed by doubling, or thickening, the sign for the short vowel; or perhaps better by combining with it the mark (–), giving –, ∠ or <, ⅃ or > for the long sounds of (·) (ˊ) (ˋ) respectively.

Scale of Vowels.

Position :—	Front.	Palatal.	Guttural.	Series.
		Short.		
Candy's Physiol. Alphabet.		ˋ ˊ ·	ˋ ˊ ·	Upper.
	ˋ ˊ	ˋ ˊ ·	ˋ ·	Middle.
	ˋ ˊ	·	ˋ ·	Lower.
Ellis's Digraphic Alphabet.		*i iə e*	*eə ae a*	Upper.
	ue uh ih	*eh oh eo*	*oe ə*	Middle.
	u uə o	*oə*	*ao aə*	Lower.
		Long.		
Candy's Physiol. Alphabet.		> < –	> < –	Upper.
	> < –	> < –	> –	Middle.
	> < –	–	> –	Lower.
Ellis's Digraphic Alphabet.		*ii iiə ee*	*eeə aae aa*	Upper.
	uue uuh iih	*eeh ooh eeo*	*ooe əə*	Middle.
	uu uuə oo	*ooə*	*aao aaə*	Lower.

Key to the Vowels.

Ph. Digr. *Short.*		Phys. Digr. *Long.*	
a,	Ital. and Germ. short *a*.	*aa*,	*a* in "father."
ae,	English short *a*.	*aae*,	Provincial English.
eə,	English short *e*.	*eeə*,	*ea* in "bear."
e,	French *é*.	*ee*,	English *ay*, French *ée*.
iə,	English short *i*.	*iiə*,	the long sound of
i,	French short *i*.	*ii*,	Engl. *ee*, Fr. *î*, Germ. *ie*.
ə,	Engl. & Dutch *u*, Sanscr. *a*.	*əə*,	the long sound of
oe,	Fr. *eu*, Ger. *oe* or *ö* short.	*ooe*,	Fr. *eu*, Germ. *oe* or *ö* long.
eo,	French mute *e*.	*eeo*,	the long sound of
oh,	Gaelic *ao* in "laogh."	*ooh*,	the long sound of
eh,	Polish *y* short.	*eeh*,	the long sound of
ih,	Welsh *y* (tongue between [teeth)	*iih*,	Welsh *y* long.
uh,	Swedish *u* short.	*uuh*,	Swedish *u* long.
ue,	Sc. *ui*, Ger. *ue* or *ü*, Sw. *y*	*uue*,	German *ue* or *ü* long.
aə,	French *a* short.	*aaə*,	French *â*.
ao,	English *o* short.	*aao*,	English *a* in "water."
oə,	Italian *o* aperto, short.	*ooə*,	Ital. *o* aperto, long.
o,	English *o* in "omit."	*oo*,	Engl. long *o*, Gr. *ω*.
uə,	Ital. *o* chiuso, Sw. *o* short	*uuə*,	Ital. *o* chiuso, Sw. *o* long.
u,	Engl. *oo* short; Germ. &c. *u* short.	*uu*,	Engl. *oo* long, Germ *uh*.

November 9, 1857.

Professor Sedgwick gave a description of a series of dislocations which have moved the Cambrian and Silurian rocks between Leven Sands and Duddon Sands, several miles out of their normal position in the Geological Map of the Lake Mountains.

To make the subsequent descriptions clear, the author first gave a normal or typical section of the older Palæozoic rocks, by enumerating (in an ascending order) the great groups which have been well established, as follows:—1. Skiddaw Slate. 2. Chloritic slate, porphyry, trappean shales, &c. 3. Coniston Limestone, calcareous

slate, Coniston Flagstone, &c. (All the above three groups are called Cambrian.) 4. Coniston Grit,—a good physical group, which however, from the paucity of its fossils, may be thought of ambiguous relations: it appears to represent the May Hill Sandstone, and therefore to be the base of those rocks which, in the north of England, represent the Wenlock and Ludlow groups of Siluria. 5. A coarse slate, often much contorted. 6. A bed of impure limestone, which may be traced from Tottlebank Fell towards the north-east for about two miles, after which it thins out and does not again appear further northward. 7. A coarse and often contorted state. When No. 6 is wanting, No. 5 and No. 7 may be considered as one group (now called Banisdale Slate), which is widely spread, and gradually passes into more coarse and gritty masses, that link themselves to the next superior group. 8. Grit, slate, and tilestone, expanded between Kendal and Kirkby Lonsdale. This is followed by unconformable masses of Old Red Sandstone and carboniferous limestone. Of the above groups, Nos. 5, 6, and 7 approximately represent the Wenlock series, and No. 8 abounds in characteristic Ludlow fossils.

In the range of these groups from Shap Fell to Tottlebank Fell (which is about two miles south-west of Coniston Waterfoot) there seems to be no ambiguity; but to the south of Tottlebank Fell the groups have, through the intervention of great *faults*, been thrown into such abnormal positions that their relations have often been misunderstood. Thus, (in 1822) when the author first attempted to map this part of Lancashire, he was led, by the line of strike as well as by the whole physical characters of the country, to identify the Tottlebank Limestone with a calcareous band a few miles further south, which ranges (on the east side of the Duddon estuary) from the hills above Bank House, through Meer Beck, towards the village of Ireleth, and which from thence by an enormous *fault* (*upcast* towards the south) is thrown into the ridge of High Haume, near Dalton, from which it is continued nearly in the same strike till it is covered by the Old Red Sandstone and Mountain Limestone. This identification was however erroneous; for the limestone-beds above mentioned, ranging on the east side of Duddon Sands, are altogether unconnected with the Tottlebank Limestone, and are in mineral type and fossils absolutely identical with that portion of the Coniston Limestone which appears, as is well known, on the other side of Duddon Sands in the south-western extremity of Cumberland.

The above mistake (made in like manner by several subsequent observers) was partially corrected by the author in 1845; when, on good physical and fossil evidence, he placed the High Haume Limestone on the same parallel with that of Coniston. Not having any fossils from the limestone quarries north of the village of Ireleth, and not having found a single characteristic fossil from the slate-rocks between Ulverston Sands and Duddon Sands, he was in 1845 unable to carry his correction any farther; but even then he remarked again and again that the calcareous beds north of the village of Ireleth in structure resembled the Coniston Limestone, which is seen on the north-west side of the Duddon, much more nearly than they resembled the calcareous beds of Tottlebank.

In the autumn of 1856 the author, accompanied by his friends (Mr. Gough of Reston Hall, and Mr. John Ruthven of Kendal), saw for the first time the excellent local collection formed during the labours of many past years by Mr. Bolton of Ulverston. It was evident almost at a glance that the fossils he had collected from the slate-rocks between Ulverston and Duddon Sands belonged to the upper part of the Coniston group (No. 3). He kindly pointed out to them some of his best localities, and they left the country convinced that nearly all the older rocks between Ulverston and the Duddon estuary belonged to the Coniston group, and consequently that these rocks were not superior, but inferior to the Coniston grits, though the prevailing dip and the geographical position of the groups might seem to indicate the very contrary.

In 1857 they again visited the district, re-examined Mr. Bolton's unrivalled local collection, and again during two days made traverses under his guidance. They then devoted a few days to the approximate determination of the vast breaks and faults, which have so much disturbed the normal position of the physical groups in a part of Furness, and made the colours of the Geological Map to appear almost incredibly anomalous.

The author then described, by help of plans and sections, the *faults* above alluded to. 1. Black Coomb, protruded as it is at the south-west end of Cumberland, seems to have been a kind of centre of disturbance. The chloritic slates and Coniston group which skirt the south-east side of Black Coomb have been ripped up by a north and south *fault* which at one cast throws the Coniston limestone about three miles to the south of its previous range.

2. Similar enormous *up-casts* towards the south-east cause the repetition of the Coniston limestone and flagstone on the other side of the Duddon Sands. This repetition is not produced by undulations, but by great *up-cast faults.*

3. A great east and west *fault* descends near the rivulet of Beck Side with a *down-cast* to the north, which brings the Ireleth slates down to the level of the sea at Sandside.

4. A complicated fault, or system of faults, with a very great *up-cast* to the south-east, runs from Kirkby Hall, skirting the brow of the hill under the great Ireleth slate quarries. The slates of Ireleth cannot be separated from the Coniston flag. They do not overlie the Coniston grits (as the author and other observers had long supposed), but abut against them. This conclusion seems inevitable, though the sections are broken and difficult of interpretation.

5. By a complication of *faults* the Coniston grits are widely expanded in the hills immediately north of the great Ireleth slate quarries; but all the above mentioned groups are by an east and west *fault* (or series of faults), with an enormous up-cast towards the south, cut off from the normal groups (viz. Nos. 4, 5, 6, and 7 of the typical section), which range towards Coniston water and thence into Westmoreland. This east and west fault runs down into the valley of the Crake, not far from Lowick Bridge.

6. Another great *fault* appears to descend from Coniston water-

head down the Crake, producing an up-cast on its south-east side.

The facts given in the above abstract necessitate a partial change of nomenclature. The Ireleth slates can no longer be appealed to as groups *superior* to the Coniston grits and on the parallel of the Wenlock shale. But in Banisdale there are old slate quarries in the group which, without sectional difficulty or ambiguity, does overlie the Coniston grits. For the future, therefore, the author proposes to use the term Banisdale slates for the lower part of the group which overlies the Coniston grits.

Having approximately laid down the *faults* above mentioned, there was still something wanting to complete the evidence; for the slate rocks between Duddon Sands and Leven Sands, in spite of their contortions, form an ascending section of great thickness. If, therefore, these slates be a repetition of the Coniston flags, the Coniston grits (typical section No. 4) might be looked for somewhere towards the south-east. To put this to the test, the author (accompanied by his two friends) made a complete traverse from Broughton to the upper part of Leven Sands; and they found, as they were finishing their traverse, that the ridges which skirt the estuary of the Leven, below Penny Bridge, were composed of the Coniston grits in their characteristic form. The evidence was then complete, and they next day left the country.

Also a paper was read by Professor De Morgan, "On the Beats of Imperfect Consonances."

This subject has been left in great obscurity by Dr. Smith, and subsequent writers have either neglected it, or misunderstood it, or obtained results by methods which miss the principal simplification of which the theory appears susceptible. Omitting historical matter, Mr. De Morgan's method may be described as follows:—

The grave harmonic of Tartini, formed by sounding two notes of which the vibrations take n and m equal parts of time ($m : n$ being in its lowest terms), has a vibration which lasts through mn of those times. This is called *Tartini's beat*, whether it produce a sound, or whether it only produce what Dr. Smith calls a *fluttering*. This beat is most perfect when the consonance is in perfect tune. If the consonance be a little out of tune, Tartini's beats are not destroyed, but do not succeed each other with perfect reiteration of circumstances, owing to the gradual advance or regression of the position in one vibration of the commencement of the other. A cycle of disturbances is the result, which cycle is repeated, or repeated *quam proxime*; and the ear recognizes this recurrence in *Smith's beats*, which are entirely due to the imperfection of the consonance. The connexion has a close resemblance to that of the instantaneous ellipse of a planet and its disturbed orbit.

The simplest connexion of beats and vibrations is as follows:— The smaller of the two numbers, n and m, being n, every vibration by which the upper note is tuned wrong gives n beats per second. Thus, the consonance being a fifth (2 : 3), every vibration by

which the upper note is too flat or too sharp gives two beats per second.

In an appendix, Mr. De Morgan gives some tables of beats, repeats some theorems on temperament from the Penny Cyclopædia, and recommends and argues in favour of tuning being performed by a *whole octave* of tuning forks, adjusted by beats to the system employed.

November 23, 1857.

A paper was read by Professor Thompson, "On the *Sophista* of Plato."

In this paper the genuineness of the *Sophista* was defended, and some of its philosophical bearings pointed out. In answer to the doubts expressed by the Master of Trinity in a previous communication, it was shown that the *Sophista*, as well as the *Politicus*, which is a continuation of it, are repeatedly referred to in the works of Aristotle. In particular, *Arist. Metaph.* v. ii. 93 was appealed to as evidence that Aristotle had not only read the dialogue called *Sophista*, but believed it to have been written by his master.

The Dialogue was analysed, and shown to be a critique of the negative or Eristic systems of logic, derived from the Eleatics, which were taught by Euclides and Antisthenes, the founders of the Socratic sects of the Megarics and Cynics respectively. Many allusions, personal and otherwise, to Antisthenes were pointed out, as existing both in this dialogue and in the *Theætetus*, of which it is a professed continuation. The *Theætetus* was regarded as a critique of the contemporary psychology, and the *Sophista* as a confutation of the prevailing schemes of logic; and both were shown to contain exemplifications of the twin proce ses of Induction or Collection, and Division or Classification, which constitute, according to Plato in the *Phædrus*, p. 265 E., the science or art of Dialectic.

It was also argued, in opposition to Schleiermacher, that the Materialistic doctrines confuted in *Sophista*, p. 246, represent those of Antisthenes, rather than the atomic theory of Democritus, or the empirical system of Aristippus.

The analysis of the simple Proposition (*Soph.* p. 262) was shown, by the testimony of Plutarch and others, to be Platonic, and the imperfect Idealists refuted in p. 276 were identified with the Megarics, and distinguished from the Platonists.

Passages were also quoted from the *Politicus*, showing the disciplinary and educational uses to which the method of Division was made subservient in the teaching of the Academy; and this teaching was further illustrated by a quotation of considerable length from a Comic Poet *ap. Athen.* lib. ii.

Incidentally, Porphyry and Abelard were appealed to in evidence that Plato's Method of Division was known to the Neo-Platonists and the Schoolmen, and recognized by them as characteristic of his Dialectic.

December 7, 1857.

Professor Miller made a communication on the Planimeters of Wetli, Decher, and Amsler, and communicated the following simple proof of the principle of Amsler's, due to Mr. Adams.

Let O be the fixed point,

P the tracer,

Q the hinge,

W the centre of wheel,

M the middle point of P Q,

$OQ=a$, $PQ=b$, $MW=c$.

The area of any closed figure whose boundary is traced out by P, is the algebraical sum of the elementary areas swept out by the broken line O Q P in its successive positions.

Let ϕ and ψ be the angles which OQ, QP at any time make respectively with their initial positions.

s the arc which the wheel has turned through at the same time.

If now OQP take up a consecutive position, and ϕ, ψ, s receive the small increments $\delta\phi$, $\delta\psi$, δs, we see that δs = motion of W in direction perpendicular to PQ.

Hence motion of M in the same direction $=\delta s+c\delta\psi$, and therefore the elementary area traced out by $QP=b(\delta s+c\delta\psi)$. Also elementary area traced out by $OQ=\frac{1}{2}a^2\delta\phi$.

Hence the whole area swept out by OQP in moving from its initial to any other position is

$$\tfrac{1}{2}a^2\phi+bc\psi+bs.$$

If OQP returns to its initial position without performing a complete revolution about O, the limits of ϕ and ψ are 0, and the area of the figure traced out by P is bs.

If OQP has performed a complete revolution, the limits of ϕ and ψ are 2π, and the area traced out is

$$\pi(a^2+2bc)+bs.$$

A paper was also read by the Astronomer Royal, "On the substitution of Methods founded on Ordinary Geometry for Methods based on the General Doctrine of Proportions, in the treatment of some Geometrical Problems."

The doctrine of proportions laid down in the fifth book of Euclid is the only one applicable to all cases without exception, but it is cumbrous and difficult to remember. It is therefore natural to attempt, in special applications of the doctrine, to introduce the facilities which are special to each case. This has been done long

since in the case of numbers, and this the author of this paper attempts in some cases in which geometrical lines only are the subject of consideration, by a new treatment of a theorem equivalent to Euclid's simple *ex æquali* and of the doctrine of similar triangles, referring to nothing more advanced than Euclid, Book II.

The author proves,—

1. If the rectangle contained under the sides a, B be equal to the rectangle contained under the sides b, A; and if these rectangles be so applied together that the sides a and b shall be in a straight line and that the side B shall meet the side A, the two rectangles will be the complements of the rectangles on the diameter of a rectangle.

2. If the rectangle contained under the lines a, B is equal to the rectangle contained under the lines b, A; and if the rectangle under the lines b, C is equal to the rectangle contained under the lines c, B; then will the rectangle contained under the lines a, C be equal to the rectangle contained under the lines c, A.

(This is equivalent to the ordinary *ex æquali* theorem.

If $a : b :: A : B$

and $b : c :: B : C,$

then will $a : c :: A : C.$)

3. If two right-angled triangles are equiangular, and if a, A are their hypothenuses, and b, B homonymous sides, the rectangle contained under the lines a, B is equal to the rectangle contained under the lines b, A.

(The equivalent theorem in proportions is

$$a : b :: A : B.)$$

4. If a, c and A, C are homonymous sides of equiangular triangles, the rectangle contained under a, C will be equal to the rectangle contained under c, A.

5. If b, c and B, C are homonymous sides including the right angles of two equiangular right-angled triangles, the rectangle contained under b, C will be equal to the rectangle contained under c, B.

6. If the rectangle contained under the lines a, B is equal to the rectangle contained under the lines b, A; the parallelogram contained under the lines a, B will be equal to the equiangular parallelogram contained under the lines b, A.

(This is equivalent to the proposition,

If $a : b :: A : B$

then $a : b :: A \cos \alpha : B \cos \alpha.$)

These propositions will suffice for the treatment of the first thirteen propositions of Euclid's sixth book (Prop. I. excepted), and of all the theorems and problems apparently involving proportions of straight lines (not of areas, &c.) which usually present themselves. The author then proceeds, as an instance of their application, to prove by means of them the following theorem :—

If pairs of tangents are drawn externally to each couple of three unequal circles, the three intersections of the tangents of each pair will be in one straight line.

Also a paper was read by Professor De Morgan, "On a Proof of the existence of a Root in every Algebraic Equation: with an examination and extension of Cauchy's Theorem of Imaginary Roots; and remarks on the proofs of the existence of Roots given by Argand and by Mourey."

The extension of Cauchy's theorem is very easily found, when the proof is the first of those given by Sturm in Liouville's Journal. The extended theorem is as follows:—

Let ϕz be any function of z, and let $z=x+y\sqrt{-1}$. Let (x, y) be a point on any circuit which does not cut itself. Let this point describe the circuit in the positive direction of revolution; and, $\phi(x+y\sqrt{-1})$ being $P+Q\,.\,\sqrt{-1}$, let $\frac{P}{Q}$ change sign k times as in $+0-$, and l times as in $-0+$. Let (x, y) be called a radical point when $\phi(x+y\sqrt{-1})=0$, or $=\infty$. Let there be m radical points of the first kind within, and m' upon, the circuit: let there be n radical points of the second kind within, and n' upon, the circuit. Then

$$k-l=2m+m'-(2n+n').$$

Sturm's demonstration of the case where $m'=0, n=0, n'=0$, which is Cauchy's theorem, assumes the existence of the roots of an algebraical expression. Mr. De Morgan's proof of the existence of these roots is as follows:—He shows, *à priori*, that in the sequence of signs which Cauchy's theorem requires to be examined, $k-l$ never undergoes any alteration except after 0 and ∞ have coincided, that is, where $P=0$, $Q=0$, simultaneously. It is then easily proved that change in $k-l$ happens in every algebraical equation.

The proofs given by Argand and Mourey were intended as illustrations of the power of the extension which is now called *double algebra*. Stript of this interpretation, they are purely algebraical, and Argand's proof is really that which was afterwards found by Cauchy. Argand's proof is more simple in form than Cauchy's.

February 8, 1858.

A paper was read by the Rev. O. Fisher, "On the probable origin of numerous Deep Pits on some Heaths in Dorsetshire."

Also a paper was read by Professor De Morgan, "On the Syllogism, No. III., and on Logic in general."

This paper is divided into two sections, the first of which is descriptive and controversial, the second is an abstract of the system.

Between the opinion of Kant that logic cannot be improved, and that of some recent writers, who hold it perverted, and not always correct, the truth is held to lie in this,—that existing logic, in its *quod semper, quod ubique, quod ab omnibus*, is true and accurate; but that it is only a beginning, and that the low estimation in which it has been held is a consequence of its incompleteness.

The modern definition of logic, the *form of thought*, relates to a distinction which is more familiar to mathematicians than to logicians, but is rather in the common use of the mathematician than in his clear apprehension. Aristotle, who first implicitly made the distinction of form and matter, was a mathematician; and so also was Kant, who first explicitly introduced this distinction into the definition of logic. The only two nations who had a logic taking character from the distinction, the Greeks and Hindus, are precisely the two nations to whom we owe the rudiments of our mathematics. It is affirmed by the author, that, in our time, the distinction is more in the theory of the logician than in his practice, more in the practice of the mathematician than in his theory.

Various illustrations are given of the manner in which recent logical writers have, according to Mr. De Morgan, misconceived the distinction of formal and material. In another part of the paper he suggests that this distinction has been confounded with the distinction which he designates as onymatic and non-onymatic. By *onymatic* he means what arises out of the use and meaning of nomenclature: thus the relation of containing and contained is an important relation of names to each other as names, or an onymatic relation.

The modern logic, by the simplicity of its *final* examples, is prevented from being of much use as a mental gymnastic. Instances are given of a proposition and a syllogism which are more worthy of being propounded as *exercises* than the instances which are found in works on logic.

The objections to symbols are discussed. Every science which has thriven has organized symbols of its own: and logic, the only science which confessedly has made no progress for many centuries, is also the only science which has grown no symbols.

The logicians have confined themselves hitherto within what Mr. De Morgan calls the *logico-mathematical* field: they now begin to contend for the inclusion of what he calls the *logico-metaphysical*. This distinction they take as that of *extension* and *comprehension*. The author contends for a distinction of *extension* and *intension* in both the sides of logic, the mathematical and the metaphysical; though undoubtedly extension predominates in the mathematical side, and intension in the metaphysical. These distinctions are onymatic. If the name C contain all that is in A or in B, or in both, symbolized by $C=(A, B)$, then A and B are in the extension of C. But if C be contained both in A and in B, symbolized by $C=A\text{-}B$, or AB, then A and B are in the intension of C.

A name is used in four senses. It is the name of an object, or of a quality inhering in an object, and distinguishing a class: these two

uses are objective. It is also the name of a class, or of an attribute by which the mind thinks of a class: these two uses are subjective. The subjective uses are reductions of plurality to unity, a description for the truth and reality of which the author contends.

It is affirmed that the logicians have not only confined themselves within the mathematical side of logic, but that even the recent attempts to introduce the metaphysical appear like attempts to create a second mathematical branch. This is evidenced by the manner in which unity of *attribution* has been discarded in favour of plurality of *qualification*. Thus it has been said, in obedience to the theory of quantification of the predicate, that the *humanity* of Newton is a different thing from the *humanity* of Leibnitz. That this view, though true, belongs to the *mathematical* side of logic, is contended for and enforced at length.

Aristotle made the distinction which the logicians now recognize as that of extension and comprehension, and which Mr. De Morgan distinguishes as that of mathematical and metaphysical reading, as follows:—In one sense the species is in the genus: in another the genus is in the species. That is, all the species are *aggregants* of the genus: the whole genus is a *component* of the notion of the species.

Recent English logicians of high name have misconceived this distinction to the extent of imagining that by changing 'Every A is some B' into 'Some B is every A,' they make the change alluded to by Aristotle. Mr. De Morgan restores the old distinction, and completely incorporates what was only partially introduced, the *change of quantity* which takes place in passing from the mathematical to the metaphysical reading. Thus 'Every A is B' is in the first reading 'The whole class A is one aggregant of the class B'; and in the second, 'The whole attribute B is one component of the attribute A.'

The limitation of the *universe* of a proposition, made throughout the author's preceding writings, is again contended for.

A proposition is the assertion or denial of a relation between two notions. Relations which are of necessity involved in nomenclature, are called *onymatic*; and these must be first studied. Mr. De Morgan believes that the logicians have described, under the distinction of *formal* and *material*, no more than the distinction of *onymatic* and *non-onymatic*. The mathematical notion of class, and the metaphysical notion of attribute, give four different readings of a proposition:—1. *Logico-mathematical*, class aggregate of class; man contained in animal. 2. *Logico-physical*, attribute predicated of class; *animality* attribute of the class man. 3. *Logico-metaphysical*, attribute component of attribute; animality a component of humanity. 4. *Logico-contraphysical*, attribute subjected to class; humanity only predicable within the class animal.

The logicians confined logical predication to the idea of class contained in class, species in genus. The genus in species, attribute component of attribute, they relegated to metaphysics. Hence their distinction of the *logical* and *metaphysical* whole. The class composed of individuals they called the *mathematical* whole: Mr.

De Morgan calls it the *arithmetical* whole, transferring the word *mathematical* to what was called the logical whole. The common mode of expression, as 'Every A is B,' &c., he considers as speaking the language of the arithmetical whole, though the speaker may attach the idea of either of the other wholes.

Extension predominates in the mathematical whole; intension in the metaphysical. The most usual mode of speech is the *physical*: man is educated a mathematician as to the subject of his proposition, a metaphysician as to the predicate.

The most remarkable point at issue between Mr. De Morgan and the logicians, is in his opposition to their notion of the whole attribute being the *sum* of its components. The difference between aggregation and composition is one of the turning-points of his whole system.

The distinctions above drawn require differences of language to express the relations which enter: the logicians have nothing but the copula *is*. At the outset, however, we have the distinction which is expressed by speaking of relations of *terminal ambiguity* and relations of *terminal precision*:— the first seen in 'A is contained in B,' where it is left unsaid whether or no A fills B; the second seen in the case in which it is implied that A is part *only* of B.

By speaking in the arithmetical whole, the logicians have made a system of syllogism from which the numerical syllogism cannot be excluded. The propositions 'Some As are Bs,' and '50 As are Bs,' are of the same kind: they are both referred to the arithmetical whole. This whole is subordinate to both the mathematical and metaphysical wholes; though more prominent in the first than in the second.

When inclusion and exclusion are opposed to one another, and combined with assertion and denial, the ordinary proposition takes a form in which quantity is but an emergent incident, and not a fundamental mode of discrimination. Thus the propositions A and O are the assertion and denial of the inclusion of class in class; E and I are the assertion and denial of the exclusion of class from class.

The opposition of the two kinds of quantity, extensive and intensive, is not easy and natural, when the word *quantity* is used in metaphysical reading. Mr. De Morgan proposes the word *force* to express quantity in the second case. He finds this word in use. Thus it is sometimes said that a term is or is not used in its complete force, when the meaning is, that all the attributes of which the term is compounded are or are not involved in the use made of the term. This is, according to Mr. De Morgan, one of the cases in which the logical system of the world at large has got beyond that of the logicians.

The *spicular* notation of the former papers is extended: the signs) and] being used to distinguish mathematical and metaphysical reading. Then X))Y signifies that the whole class X is contained in the class Y; X]]Y signifies that the whole attribute Y is a component of the attribute X.

The syllogism is the deduction of a relation from the combination of two others. By distinction of the mathematical and metaphysical,

of the terminally ambiguous and terminally precise, four modes of combination are obtained. Logicians have but the copula *is* for all cases. Mr. De Morgan proposes to use a complete system of *schetical* terms, by which the combination of relations shall be exhibited. Leaving out the cases of terminal precision, which are more complex and less usual, the two kinds of reading under which the common syllogism is included are as follows:—

Terminal Ambiguity. Mathematical reading.

Relation of Class X to Class Y.

The class x is the *contrary* of X, or contains all the rest of the universe.

Proposition.	X—of Y.	Y—of X.	Notation.
Assertion of X contained in Y	Species	Genus	X))Y
Denial of X contained in Y	Exient	Deficient	X(.(Y
Assertion of X excluded from Y	Coexternal	Coexternal	X).(Y
Denial of X excluded from Y	Copartient	Copartient	X()Y
Assertion of x contained in Y	Complement	Complement	X(.)Y
Denial of x contained in Y	Coinadequate	Coinadequate	X)(Y
Assertion of x excluded from Y	Genus	Species	X((Y
Denial of x excluded from Y	Deficient	Exient	X).)Y

Terminal Ambiguity. Metaphysical reading.

Relation of attribute Y to attribute X.

	Y—of X.	X—of Y.	Notation.
Assertion of Y a component of X	Essential	Dependent	X]]Y
Denial of Y a component of X	Non-essential	Independent	X[.[Y
Assertion of Y incompatible with X	Repugnant	Repugnant	X].[Y
Denial of Y incompatible with X	Irrepugnant	Irrepugnant	X[]Y
Assertion of Y a component of x	Alternative	Alternative	X[.]Y
Denial of Y a component of x	Inalternative	Inalternative	X][Y
Assertion of Y incompatible with x	Dependent	Essential	X[[Y
Denial of Y incompatible with x	Independent	Inessential	X].]Y

The extension of the four forms to eight, the notation, &c., are treated in the second paper on syllogism. The two sets contain the same propositions, differently read; and the quantities in the two are different. In the first reading X) and (X denote X taken universally in extension; X(and)X denote X taken particularly. In the second reading]X and X[are universals, X] and [X are particulars. Thus, when we say that the classes X and Y are copartient, or in common language 'some Xs are Ys,' denoted by X()Y, both X and Y have particular *quantity* in extension. In saying this we also say that X and Y, as attributes, are irrepugnant, or not incompatible, denoted by X[]Y. But the intensive *force* of both X and Y is universal; no one attribute of X is repugnant to any one attribute of Y.

The syllogism denoted by X))Y)(Z contains the assertions that X is a genus of Y and Y a coinadequate of Z, (Y and Z not together filling the universe). The conclusion is X)(Z, X is a coinadequate of

Z, and the combination of relations is seen in—Every species of a coinadequate is a coinadequate. In metaphysical reading, we have X]]Y][Z, X is a dependent of Y, Y an inalternative of Z. The conclusion is X][Z, X is an inalternative of Z, and the combination of relations is seen in—The dependent of an inalternative is an inalternative. When the terms become as familiar as genus and species, the axiomatic character of the combination is as clearly manifest as in—Species of species is species. Mr. De Morgan gives the following instance of a good inference which would probably not be seen with ease in its present form, though the phrases are not technical: "We must not say that either bodily strength or meanness is a necessary alternative, for courage and meanness are incompatible, while courage does not depend on bodily strength." And he maintains that the educated world has made considerable advance in the use of relations of attributes, though the logician has nothing but what he calls the arithmetical *abacus* on which to exhibit the process.

Some modern logicians have so completely fallen into the mathematical view of quantity, that there is a school which treats all thought as relation of more and less. Mr. De Morgan opposes this view.

The second part of this paper, being a non-controversial summary of Mr. De Morgan's system, so far as *onymatic* relations are concerned, hardly admits of abstract. Its principal points have been touched on.

In a postscript, such notice is taken of the late Sir W. Hamilton's criticism on Mr. De Morgan's second paper as circumstances require and will allow.

February 22, 1858.

Dr. Donaldson, of Trinity College, read a paper "On the Statue of Solon mentioned by Æschines and Demosthenes."

The object of the author of this paper was to fix the age and subject of a beautiful statue in the *Museo Borbonico* at Naples, which was recovered from the ruins of the theatre at Herculaneum. This statue has generally been regarded as representing Aristides the Just, the son of Lysimachus; and one attempt has been made to show that it is a portrait of Ælius Aristides, the rhetorician, who was born 38 years after the destruction of Herculaneum in A.D. 79. A more plausible hypothesis, supported by great names, considers the statue as a portrait of Æschines. But this rests on a palpable misconception. After refuting these theories, the author undertook to show that the statue was probably a copy of that erected in honour of Solon in the agora at Salamis, and mentioned in a striking manner by Æschines (c. *Timarch.* p. 4) and Demosthenes (*De Fals. Leg.* p. 420). This was argued from the peculiar and distinctive attitude; from the fact that the treatment of the drapery accorded with that belonging to the school of Scopos, and the costume corresponded to that of the epoch (about fifty years before B.C. 343) assigned to the statue of Solon by Demosthenes; and from the suitableness of a statue of Solon, who was an elegiac

poet as well as a legislator, to the place where the statue was found, namely, the theatre at Herculaneum. Attention was also directed to the improbability of a later appropriation of a statue in such a peculiar posture, and Dion Chrysostomus was cited to show that even the Rhodians, who had adopted the practice of altering the inscriptions of honorary statues, abstained from interfering with those which were defined, not only by the name, but by the characteristics of the person represented.

March 8, 1858.

A paper was read by Dr. Paget, "On some Instances of remarkable Defects in the Voluntary Muscles."

Four original cases, in which large and important muscles, such as the pectorals, were wholly absent or in a state of extreme tenuity; the defects either congenital, or existing from early infancy; limited to certain groups of muscles, and unaccompanied with any defect or deformity of the bones. In three of the cases, the effects symmetrical; in the fourth confined to one side of the chest. Enormous development of the calves in one of the cases.

Also a paper was read "On Organic Polarity," by H. F. Baxter, Esq.

The object of the paper is to show the intimate connexion that exists between *organic* force and the ordinary *polar* forces, such as chemical force, for example.

The principal experiments, showing that organic action, viz. *secretion*, is accompanied with the manifestation of current force, have already appeared in the Royal Society's 'Transactions' for the years 1848 and 1852; but in the present communication the author enters more minutely into the resemblance between the actions which take place in the voltaic circle and those that occur during secretion than could be prudently attempted in his previous papers. But whatever view may be entertained in regard to the *origin* of the power in the voltaic circle, whether by mere *contact* or by *chemical action*, the decision of this point is of no importance to the question under consideration; since the *manifestation* of current force *during* voltaic action is allowed both by the *chemical* theorist as well as the *contact* theorist; and if we admit the manifestation of this force (*current force*) to be evidence of polar action in one class of cases, viz. during voltaic action, we are certainly justified in logically concluding that it may be adduced as evidence of polar action in other cases also, viz. *during* organic action as in secretion. Reference is made to Prof. Graham's researches on *osmose*. According to Prof. Graham, *osmose* would appear to be dependent upon *chemical action*, and consequently, should we be disposed to class the phænomena of secretion with those of *osmose*, we should be thus compelled to acknowledge that the act of secretion must be polar in its nature.

The author does not attempt to show in what manner secretion is effected, his great object being to point out what does occur during this act, viz. the manifestation of *polar* action, and consequently it is upon this ground that we may logically infer that the *force* must be POLAR in its nature.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

April 26, 1858.

Professor Challis made a communication "On the Annular Eclipse of the Sun, March 15, 1858."

The Master of Trinity read a paper "On Barrow, and his Academical Times."

May 10, 1858.

Professor Miller made communications—(1) "On an improved Method of finding the position of any face of a Crystal belonging to the Anorthic System." (2) "On the direction of the Axis of a Zone." (3) On a substitute for the Reflective Goniometer."

Mr. Godfray read a paper "On a Chart and Diagram for facilitating Great-Circle Sailing."

It has long been known that in many cases a *very great* saving of distance would be effected by guiding the ship along the arc of a great circle instead of following the rhumb; but the tedious calculations which great-circle sailing requires, and the difficulty of tracing the track on the chart, have hitherto stood in the way of its adoption. The great advantage which Mercator's sailing offers in these respects sufficiently explains the preference given to it, even by those who are fully aware of the longer route it obliges them to follow.

The object of this communication was to show how the same advantages are secured to great-circle sailing by the adoption of a chart on the central or gnomonic projection, which, with the addition of a diagram, solves the problem of this sailing with the same facility as Mercator's chart does for sailing on a rhumb.

The great-circle track becomes a straight line joining the two places; and this being drawn, the various courses and the distances to be run upon each are obtained, as also the distance from the ship to her destination, by a mere inspection of the diagram and without calculation.

The great-circle track is not always practicable, on account of its taking the ship into too high a latitude, where the ice would render it dangerous or impossible to penetrate. When this is the case, the same chart and diagram will, with just as much facility as before, point out that which, under the circumstances, is the shortest route. Some parallel of latitude is fixed upon for the maximum, and the track to be followed will then consist of a portion of that parallel and of the portions of two great circles which are tangents to it,—one passing through the ship, the other through the destination. On this great-circle chart the track will be the two straight lines drawn from the two places, so as to touch the circle of highest latitude and the part of this circle between the points of contact.

The paper explains the construction of the chart and diagram, and illustrates their use by two examples: the first, from the southern extremity of Africa to Perth in Australia, which shows a gain of 204 miles; the other, from 30° S. lat., 18° W. long. to Melbourne, gives a gain of 1130 miles, without going into a higher latitude than 55° south.

A chart and diagram on this principle have been engraved by the Hydrographic Office, Admiralty.

May 24, 1858.

The Master of Trinity read the conclusion of his paper "On Barrow, and his Academical Times."

November 8, 1858.

Professor Challis made a communication "On Donati's Comet."

November 22, 1858.

The Public Orator gave a lecture on "The Battle of the Trebbia," the object of which was to compare the different authorities, and to illustrate them by a description of the neighbourhood of Piacenza, which he had recently visited. He showed that Polybius's narrative was the most valuable, as being in all probability the original source of all subsequent accounts, and as deriving an especial interest from the author's personal knowledge of the localities. Livy in the main followed Polybius, amplifying and varying the details rather with a view to an effective and ornamented style than to the actual truth of his statements. Some particulars, however, appeared, in this part of his work as elsewhere, to have been borrowed from L. Cincius Alimentus, or some other historian of the Second Punic

War. Cornelius Nepos, from his brevity, and Appian, from his inaccuracy, were not worthy to be taken into account. No modern historian appeared to have visited the place. The principal points to be determined were (1) the situation of Scipio's camp after he had abandoned his position in the immediate vicinity of Placentia, and (2) the identification of the deep watercourse where Hannibal placed Mago with two thousand men in ambush on the morning of the battle.

Scipio's camp was, beyond doubt, at or near Rivalta, a castle and hamlet situated on a 'high bank' (as the name imports) on the further side of the Trebbia, about nine miles south-west of Piacenza. The ambuscade was placed in the watercourse called the Trebbiola, a small stream, of which the banks were from 6 to 8 feet high, about six miles from Piacenza, above the place called 'La Volta di Settima.'

The passages of Polybius, to which reference was made in the lecture, are in Book III. chapters 66 *sqq.*; those of Livy, in Book XXI. chapters 47, 48, 52–56.

The rude plan given below may make this abstract more intelligible.

December 6, 1858.

"Suggestion of a proof of the Theorem that every Algebraic Equation has a Root." By G. B. Airy, Esq., Astronomer Royal.

In this paper the equation to be discussed is expressed under the form

$$a_\alpha r_\theta^n + b_\beta r_\theta^{n-1} + \ldots + m_\mu = 0,$$

where $a_\alpha = a(\cos\alpha + \sqrt{-1}\sin\alpha)$, $r_\theta = r(\cos\theta + \sqrt{-1}\sin\theta)$, . . . or,

as it may be written, $P+\sqrt{-1}Q=0$; and the object is to show that there will be at least one value of r between 0 and positive infinity, and one value of θ between 0 and 2π, which, used in combination, will make both P and Q $=0$.

This is effected by constructing two curves whose common abscissa is θ, and whose ordinates are respectively the corresponding values of P and Q, produced by substituting in their expressions the same value of r, and observing the change which takes place in the form of these curves, and in the position of their points of intersection, as r successively assumes all values from 0 to positive infinity. The existence of a root will be indicated by a point of intersection of these curves (the P-curve and Q-curve, as they may be called) falling on the axis of abscissæ. When $r=0$, each of these curves will be a straight line parallel to the axis of abscissæ. When $r=\infty$, the corresponding values of P and Q will generally be indefinitely great; but by reducing their values in the same proportion, which will not affect the validity of the demonstration, the Q-curve will become a line of sines, and the P-curve a line of cosines, or a line of sines drawn back through $\frac{\pi}{2}$. On constructing these curves, which we may call respectively P(0), Q(0), P(∞), (Q ∞), it will be remarked—

(1) That P(0) and Q(0) do not intersect.

(2) That P(∞) and Q(∞) intersect in two points.

(3) That one of these points of intersection is above the line of abscissæ, and the other is below it.

On considering the change in the forms of the P-curve and Q-curve as r increases from 0 to infinity, it will be seen that the P-curve must have intruded on the Q-curve, at first by simple contact; and that, as the intrusion advances, the simple contact is changed into two intersections, which will at first be on the same side of the line of abscissæ. But as, where r is indefinitely increased, any two consecutive intersections necessarily lie on opposite sides of the line of abscissæ, it may be shown, by considering the various ways in which the intrusion may take place, that in all cases one at least of the intersections must have crossed the line of abscissæ during the increase of r; and a root is thus determined.

A communication was also made by Professor Miller "On the contrivances employed by M. Porro in the construction of instruments used in Surveying and Astronomy."

February 14, 1859.

Mr. Humphry made a communication "On the Limbs of Vertebrate Animals."

He gave a brief description of the fore and hind limbs in the

several vertebrate classes, directing attention to the tripartite division of their distal segments, and to the uniformity of plan upon which they are constructed. He argued that this uniformity has relation to the mode in which their development proceeds, and to the similarity of their functions rather than any adhesion to an "ideal archtypal pattern;" and expressed his belief that the existence of such an ideal in the minds of anatomists proves in some measure a hindrance to the full study of the laws which regulate the formation of animal bodies.

The differences between the fore and hind limbs were shown to depend,—*first*, upon the fact that the hind limbs are required to propel as well as to support the trunk, and less variety of movement is needed in them; hence they are larger and firmer, and the same bone of the leg forms a main constituent both of the ankle and of the knee-joint; *secondly*, in walking and running the fore limb is extended in front of the trunk, and draws the latter after it during its flexion, whereas the hind limb is bent up beneath the trunk and drives the latter on before it during its extension. This antagonism in the mode of their action leads to an antagonism in construction of the two limbs,—of their upper segments at least, the posterior aspect of the one corresponding with the anterior aspect of the other. The antagonistic relations are brought about by a partial rotation in the long axis of the two limbs which takes place in opposite directions during development; and coincident with the rotation of the proximal segments of the fore limb in one direction is a rotation of its distal segment in an opposite direction, so as to turn the palm towards the ground.

Reasons were given for regarding the scapular and pelvic arches as formed by modifications of the hæmal, and not of the pleural, parts of the vertebræ; and for believing that the scapular arch belongs, not to the occipital bone, but to the vertebræ of the fore part of the chest or of the hinder part of the neck.

February 28, 1859.

Dr. Donaldson read a paper "On Plato's Cosmical System as exhibited in the tenth book of 'The Republic.'"

The author first gave a translation of the whole passage (Plato, *Resp.* x. 616 B, 617 E), accompanied by a critical and philological examination of the Greek text. He then undertook to show the connexion between the fanciful picture of the universe which Plato has here given, with his other speculations on the origin of things, and especially with the occult philosophy of numbers. And he argued, finally, that the tradition preserved by Clement of Alexandria, which identifies Er, the son of Armenius, with Zoroaster, rests upon a foundation in fact; and that while there is good reason to believe that the doctrines of Heracleitus and Zoroaster agreed in many essential particulars, and that Plato was well acquainted with the speculations of the Ephesian philosopher, there are certain particulars in

the cosmical myth of 'The Republic' which agree exactly with the theories known to have been common to Zoroaster and Heracleitus.

March 14, 1859.

A paper was read "On the general principles of which the Composition or aggregation of Forces is a consequence." By Prof. De Morgan.

This paper examines the fundamental grounds of the composition or, as Mr. De Morgan calls it, *aggregation* of forces. By a *tendency* is meant anything which has both *magnitude* and *application*: by *application* is meant any notion which, not presenting the idea of magnitude, presents the idea of *opposition*. Two tendencies have a third tendency for their *aggregate*, to which they are jointly equivalent: and *equivalence* is any notion which, given that things equivalent to the same are equivalent to one another, satisfies the following postulates, which are the grounds of every method of aggregation known in mechanics.

1. Any two tendencies have one aggregate (0, the aggregate of counteraction being included among possible cases), and one only.

2. The magnitude of the aggregate, and its application relatively to the applications of the aggregants, depend only on the relative, and not on the absolute, applications of the aggregants.

3. The order in which tendencies are aggregated produces no effect either on the magnitude or application of the aggregate.

4. Tendencies of the same or opposite applications are aggregated by the law of algebraical additions.

From these postulates follow the following theorems:—

5. In any aggregate, the result of partial aggregation may take the place of its own aggregants.

6. Two tendencies cannot counteract one another unless they have equal magnitudes and opposite applications.

7. An aggregate has not more than one pair of aggregants, when the applications of the aggregants are given, and are different.

8. If the aggregants be altered in any ratio, without change of application, the aggregate is altered in the same ratio, also without change of application.

9. Any tendency may be disaggregated into two of any two different applications, neither of which is its own.

From the preceding it is proved,—

1. When by application is meant *direction*, the law of aggregation must be the well-known law of the aggregation of forces meeting at a point.

2. When by application is meant *choice of a point through which a given direction is to be drawn*, the law of aggregation must be the well-known law of aggregation of parallel forces.

In the case of translations and rotations, the postulates are all laws of thought; in the case of pressures, whether divergent or parallel, whether equilibrating or producing motion, all the postu-

lates contain results of experience. Accordingly, the multifarious proofs of the laws of aggregation, in the case of pressures, are not the mathematical playthings which they are often supposed to be from their grounds being insufficiently stated. If to the postulates necessary to make the law of aggregation a consequence, be added the following, "the velocity due to the aggregate of pressures in one given direction is the aggregate of the velocities due to the pressures taken separately," it follows, as a mathematical consequence, that the pressure varies as the velocity created by it in a given time. This great law of dynamics, therefore, is not fundamentally distinct from the laws of aggregation, but follows from them with the addition of a postulate more simple than itself.

A proof was also given by Professor Stokes, of the theorem that "Every Equation has a Root."

May 2, 1859.

Professor Miller made a communication "On the employment of the Gnomonic Projection of the Sphere in Crystallography."

May 16, 1859.

Mr. Hopkins gave a lecture "On Glacial Theories."

De Saussure attributed the motion of glaciers to their sliding over the bottoms of the valleys in which they exist, but did not appear to have made any observations on the change of form to which the glacial masses may be subject during their motion. No advance was made in our knowledge of glaciers for many years after De Saussure's death, till certain Swiss observers, Charpentier and others, rather more than twenty years ago, added much to our knowledge of the subject. Among these glacial observers, M. Agassiz, with characteristic zeal and activity, soon afterwards took a prominent position. He and those who were associated with him, or had preceded him, brought forward incontestible evidence of the former extension of glaciers in the Alps, and of their efficiency in the transport of enormous angular fragments of rock from their original sites to other localities, not only in the same Alpine valley, but even on the flanks of the Jura on the opposite side of the great central valley of Switzerland. These were the great facts which bore upon speculative geology. Glaciers were still engaged in the work of transport, and they had been so on a much larger scale at some former epoch; the masses of ice of which they were composed had been then of much larger dimensions, and consequently the mean climate of Western Europe must have been at that period considerably lower than at present. No one was so active as M. Agassiz in pressing these facts on the notice of geologists, or insisted more strongly on their geological importance; and by the energy of his own character, his great reputation, and extensive personal acquaintance with men

of science, he had undoubtedly made the first great steps in giving currency to the glacial theories of geology—theories which, though viewed at that time with much distrust, had since, with such modifications as enlarged knowledge and sober judgment had imposed on them, been universally recognized by geologists.

For his unflinching advocacy of the glacial theory in its broad outlines, geologists had been unquestionably much indebted to M. Agassiz; but in his physical theories respecting glacial phænomena, he had not shown that caution or acquaintance with physical science which the subject demanded. With respect to the motion of glaciers, it may be sufficient to state that he regarded it as due to the infiltration and subsequent freezing of water within the glacier, and a consequent expansion of its mass, by which the glacier in general, and especially those portions near its lower extremity, were urged forwards in the direction in which the bed of the glacier descended. Few persons ever received this theory, and it is no longer considered as deserving of serious attention.

A few years later Professor Forbes commenced his researches among the Alpine glaciers. His 'Travels through the Alps' was published in 1843, and contained a greater amount of well-arranged information respecting glacial phænomena than perhaps all other works together on that subject. But in this lecture, Mr. Hopkins remarked, he professed to deal with theories, and not with descriptive details. M. Agassiz's second work on glaciers, his *Système Glaciàre*, also appeared in 1847. Professor Forbes introduced a new view of the motion of glaciers, which he attributes to a certain facility with which he supposes glacial ice to be capable of changing its form under the pressures to which it is subjected, in a manner similar to that in which a *viscous* mass would change its form under the same circumstances. Hence it was called the *viscous theory*. It was founded on the fact (distinctly ascertained, Mr. Hopkins believed, the same year, both by Agassiz on the glacier of the Aar, and by Prof. Forbes on the Mer de Glace) that the central portions of a glacier moved considerably faster than its lateral portions, as a viscous mass would move along a trough inclined at a small angle to the horizon; and, moreover, it was obvious that the general mass of a glacier did so change its form as to accommodate itself to the changing dimensions of the valley down which it moved.

On the other hand, it was contended that a substance so hard and brittle as glacial ice could not be said to have the property of *viscosity*, and that the different velocities of the central and lateral portions of a glacier, and the changes of form which the general mass might undergo, were more attributable to the formation of crevasses and to discontinuous ruptures of the mass, than to any continuous change of form in each infinitesimal portion of it, like that which takes place in a mass which can be properly termed viscous. That this view was partly true was obvious, since ruptures and crevasses were actually formed by the unequal motions of different portions of the mass. Those who maintained this latter view held that the glacier moved by actually *sliding* over its bed; while those who sup-

ported the *viscous theory* contended that there was no such sliding motion, or if it existed at all, it constituted but a small part of the whole observed progressive motion of the surface of the glacier.

In the warmth of discussion these theories came to be considered as more antagonistic than they really were. It was manifestly possible that the lower surface of the glacier might *slide*, and thus cause a part of the observed motion of the upper surface; which might also have an additional motion, due to the more rapid progression of the upper portions of the mass as compared with that of its lower portions retarded by friction, as in the case of a semifluid mass. On this point Mr. Hopkins quoted the following passage from one of his letters "On the Mechanism of Glacial Motion," addressed to the editors of the Philosophical Magazine in 1844–45. If observations "should concur in showing an approximate equality in the motions of the upper and lower surfaces of a glacier, every candid and impartial mind must admit, I conceive, the *sliding* in preference to the *viscous* theory; but if, on the contrary, it should be proved that the velocity of the upper bears a large ratio to that of the lower surface, the claims of the latter theory must be at once admitted." Since this was written, several observations had been made by different persons, which agreed in showing that the upper surface of a glacier does move faster than the lower surface; but the only observations Mr. Hopkins had met with which enabled us to compare the actual amounts of those motions, had been made by Prof. Forbes himself near the extremity, Mr. Hopkins believed, of one of the glaciers at Chamouni. The result was that the upper surface moved about twice as fast as the lower one, thus proving that in this instance the motion of the upper surface was due in nearly equal degrees to the two causes above mentioned, and that both theories had so far equal claims to be admitted.

But at present no one probably doubted the fact of the whole motion of a glacier being made up of that motion which it derives from the property hitherto usually designated as the *viscosity* or *plasticity* of its mass, and that which consists of a *sliding* over its bed. Dr. Tyndall had recently observed proofs of this latter motion in various parts of glaciers as well as near their lower extremities; and all the phænomena of polished and striated rocks indicate most clearly that such motion must have existed in the ancient glaciers to which such phænomena are referred. But how was it conceivable that a glacier should thus *slide* over a surface on which there must be many and considerable inequalities, and at inclinations sometimes not exceeding 2° or 3°? And if it did thus slide, how was it that it did not move, as bodies ordinarily move down inclined planes, with an *accelerated* motion? These questions were frequently dwelt upon formerly. They were completely answered by the experiments made by Mr. Hopkins, and described by him in the 'Transactions' of this Society in 1844 (vol. viii. part 1), and in his first letter "On the Motion of Glaciers," dated November 19 of that year, and inserted in the Philosophical Magazine. The motion in question was not at all analogous to that of a body descending down

an inclined plane and retarded by friction as a constant force; it was due to the fact of the cohesion of the constituent particles of the mass at its lower surface being insufficient to resist the tendency of such an enormous weight of ice to descend down a plane even of very small inclination. A continuous disintegration is thus produced, promoted probably, in a greater or less degree, by a constant but very gradual thawing of the ice at the lower surface. In this manner it is easy to see that the motion must depend on the rate of disintegration, and therefore must be nearly a uniform, and not an accelerated motion.

Professor Forbes had an undoubted claim to the credit of being the first to suggest and insist upon the capability of the general mass of a glacier to change its form under existing conditions, as a cause of glacial motion. The above explanation of the sliding of the mass Mr. Hopkins claimed for himself.

Still it was felt that further investigation was required respecting the property of glacial ice which had been designated as its *viscosity*. There was no conclusive evidence that glacial ice would bear any considerable *extension* without breaking, for numberless crevasses were formed wherever the ice appeared to be subjected to any great extending force. Again, it was equally certain that the contiguous portions of a dislocated glacial mass, though retaining their perfect solidity, did become reunited into one continuous and unbroken mass. These facts were not sufficiently explained by the assertion that glacial ice was *viscous*. The true explanation appeared to have been afforded by an observation made some time ago by Dr. Faraday, and the more recent experiments of Dr. Tyndall. The former observed that two pieces of ice in perfect contact would freeze together so as to become one perfectly continuous mass, though the surrounding temperature should be much higher than 32°; and the latter gentleman had shown, by a striking form of the experiment, the extreme facility and rapidity with which a piece of common ice, after being crushed and broken into numberless fragments, will reunite into one continuous mass of transparent ice. This process had been designated by the term "*regelation*;" and manifestly some corresponding term was required to designate the property which, in ice, rendered that process so complete. Such terms as *viscous* and *plastic* failed to express adequately the property in question. At the same time it should be remarked that, so far as glacial motion depended on the facility with which the glacial mass might change its form, the manner in which that change was effected was of secondary importance, and did not diminish whatever value attached to Professor Forbes's first recognition of this change as an important cause of glacial motion. In one respect, however, the mechanism of the motion would be in some measure affected. Dr. Tyndall contends, and in a paper recently presented to the Royal Society has collected a considerable amount of evidence to show, that glacial ice would bear no more linear extension, independently of lateral compression, than ordinary specimens of ice would lead us to suppose, and consequently, when acted on by extending forces, it

cracked, forming fissures and crevasses to a much greater extent than would seem consistent with any property to which the term *viscous* could be applied with strict propriety.

Professor Forbes was also the first to make known to us, by systematic and well-directed observations, the facts and laws of the *veined structure* in glacial ice. He also entered into elaborate speculations on the causes which produced this structure, both in his 'Travels in the Alps,' and in letters written subsequently. Dr. Tyndall has also put forward a theory suggested by an analogy between the veined structure of ice and the lamination of rocks. As there appeared to have been some confusion as to the differences between these two theories, Mr. Hopkins would endeavour, as far as he was able, to explain them.

Both these theories depended primarily on the internal tensions and pressures to which the glacial mass might be subjected. The different parts of a glacier, as was well known, move with different velocities, the most general law being that the central move faster than the lateral portions; but whatever may lead to this unequable motion, its manifest result must be a tendency to *drag* the slower-moving portions of the ice after those which move more quickly. Moreover, it was easy to see that, in certain directions, this dragging might be greater on one portion of the ice than on a contiguous portion, and might thus tend to give different motions to contiguous *vertical slices* of the mass. This difference of motion, or *differential motion*, was supposed by Professor Forbes to actually exist, and that ruptures or breaches of continuity were absolutely produced between these vertical thin slices of ice by the strain upon them, and that these ruptures gave rise to the veined structure. His first idea appeared to have been that water infiltrated into the small fissures thus formed, where it afterwards froze and formed the veins of blue transparent ice, while the intermediate vertical laminæ contained a sufficient quantity of air-bubbles to render them white and opake. This notion of infiltration appeared to have been subsequently given up by the Professor, the conversion of the opake into transparent ice being supposed to take place by pressure and differential motion, independently, as far as Mr. Hopkins understood, either of infiltration or the melting of any portions of the ice.

Both Professor Forbes and Dr. Tyndall had endeavoured to elucidate the phænomena of glacial motion by means of a semifluid substance descending down a trough inclined to the horizon. For the purpose of ascertaining the direction of greatest extension and compression of the substance when thus put in motion, the latter gentleman described circles on its surface while still at rest, and observed the compressions and extensions of the radii when the mass was in motion. He thus found that the lines of greatest extension were inclined at angles of 45° to the axis of the trough; each such line pointing centrally and downwards, or laterally and upwards, while the lines of greatest compression were perpendicular to them. In the case of a glacier descending a canal-shaped valley, the former of these directions would manifestly be that of greatest tension;

and this is precisely the result which Mr. Hopkins had obtained both by exact mechanical reasoning and by experiment fifteen years ago; and it was thus that he was able to explain (and he was the first to do so) the formation of crevasses making angles of 45° with the axis of the glacier, and directed centrally and upwards, *i. e.* at right angles to the lines of greatest tension. There was also another result to which Mr. Hopkins was led by an exact consideration of the problem, but which he had also elucidated by experiments, as described in the letters above alluded to in the Philosophical Magazine. There were not only directions of maximum and minimum pressures and tensions at each point of the mass, there were also two other directions inclined at 45° to the former, not recognized by either of the above-mentioned experimenters, in which there is a maximum tendency to produce the *differential motion*, to which Prof. Forbes ascribed an actual rupturing of the ice and consequent formation of the veined structure. In the directions of maximum and minimum pressures or tensions, this tendency to produce a differential motion altogether vanished. The truth of these results was just as certain as that of the parallelogram of forces. In more complicated cases than that above supposed of a glacier descending down a canal-shaped valley, the absolute directions of these different lines would vary with the conditions of the glacial mass, and the external pressures to which it was subjected; but the important fact was, that there must in all cases exist at each point of the mass a direction of *maximum tension* or of *minimum pressure*, and a direction, perpendicular to it, of *minimum tension* or *maximum pressure*, and two other directions inclined to each of the former at 45°, along which there is a maximum tendency to produce the kind of differential motion above described. It was moreover manifest that where crevasses were formed there must be tension; and equally manifest that the directions of such crevasses must at least approximate to *perpendicularity with the directions of maximum tension*, and therefore to coincidence with those of maximum pressure. Also, if discontinuities and differential motions resulted from these internal pressures and tensions, they must be produced in those directions in which there is the maximum tendency to produce them, *i. e.* in directions inclined at 45° to those of the crevasses. All these directions might be supposed to be (as they would generally be in a glacier) nearly horizontal. These conclusions, Mr. Hopkins repeated, were as certain as that of the parallelogram of forces, and no theory which contradicted them could possibly be true.

Professor Forbes was the first to recognize the law which establishes a certain relation between the veined structure and the crevasses. He asserts that, as a matter of observation, the crevasses intersect the structure at right angles; consequently the blue veins must be perpendicular to the directions of maximum pressure, and could not coincide (such being the law) with the directions in which *differential motion* must necessarily take place, if it should take place at all. The law above enunciated exactly accorded with the conclusions above stated, and also with Dr. Tyndall's views, who asserts,

from his own observations, that the laminæ of blue and white ice (and especially in those places in which the structure originates) conform to the law of *perpendicularity to the direction of greatest pressure.* So far from there being any tendency to produce ruptures and fissures lying in the planes of the laminæ in these positions, they were the only positions entirely free from such tendency. And hence it became so difficult to conceive how the laminated structure could possibly originate in actual discontinuities such as those to which Professor Forbes had ascribed them, whether we suppose the blue laminæ to be produced by subsequent infiltration or any other process.

According to Dr. Tyndall's views, the law stated in the preceding paragraph was an essential consequence of physical causes, to which the production of the laminæ was referred. He had shown, experimentally, that if a piece of ordinary ice be subjected to direct pressure, it will melt along fine lenticular laminæ perpendicular to the direction of the pressure. A similar process is supposed to take place in the *névé*, or in any part of the glacier where the structural laminæ originate. Professor W. Thomson had offered an explanation of this phænomenon on thermal principles. It had been shown by himself and his brother that the melting temperature of ice is *lowered* by compression. Now if, in Dr. Tyndall's experiment, the ice were a perfectly homogeneous substance, every portion would be equally compressed; and if the uncompressed mass were only just above the melting temperature, the whole would melt under the compressing force. But no substance is perfectly homogeneous; and consequently the internal pressures in the experiment would not be perfectly equable; and those portions of the ice which were subjected to the greatest pressure would melt the soonest, and produce the aqueous laminæ above mentioned in the experiment, or the laminæ of blue ice in the glacier. In the latter case the laminar portions must first be supposed to melt, the air-bubbles to escape, and the water subsequently to be refrozen to form the blue transparent laminæ.

Mr. Hopkins did not profess to maintain the entire adequacy of the above explanation of the formation of the laminated structure, though he could not but feel persuaded that it was founded in truth. It seemed to explain very satisfactorily the conversion of *névé*, or opake white ice, into transparent blue ice; but he did not well understand how transparent consolidated ice could be converted by pressure into white opake ice. But still, at the bottom of an ice-fall, as that of the glacier of the Rhone, the broken fragments were again united into a continuous mass, and the laminated structure was reproduced on a type entirely new, and conformable to the altered conditions of the mass; and, assuming a large portion of the ice descending the fall to be in a sufficiently consolidated state, the process of reconstruction must consist as much in the conversion of blue ice into white, as of white ice into blue. Mr. Hopkins was not aware whether this difficulty had been previously started, or, if so, what answer had been made to it. His object was more especially to

point out the essential distinctions between Professor Forbes's and Dr. Tyndall's theories respecting the laminar structure. A disposition had recently manifested itself to confound the two theories, whereas they were so fundamentally different, that the physical reasoning essentially involved in the one was totally inapplicable to the other. The differences were such as could not be ignored, if we would hope to arrive at a complete and final view of the subject.

The remarks in this lecture, Mr. Hopkins said, had been made with a sincere desire of eliciting the truth, and not in the mere spirit of advocacy of preconceived opinions; nor would it, he conceived, be inconsistent with this assertion if, in conclusion, he reminded those who were interested in the subject, that though his own investigations nearly fifteen years ago respecting the internal pressures and tensions of glacial masses were little noticed then, and had been little mentioned more recently, no one had ever attempted to refute them; and now, on the contrary, all those observations and experiments of Dr. Tyndall which related to this part of the subject, and were at present generally received, were entirely confirmative of them. The nature of the reasoning which has now been applied to the subject, whether founded on analogies with certain phænomena of lamination, or on thermal principles, clearly proved the necessity of more accurate conceptions of these internal pressures and tensions than could ever be acquired from merely elucidatory experiments.

May 30, 1859.

"On the Occultation of Saturn by the Moon on May 8, 1859." By Professor Challis.

In observing this occultation, Professor Challis was prepared to take especial notice of the occurrence of any phænomenon like that witnessed at the occultation of Jupiter on January 2, 1857, on which occasion the disc of the planet at emergence was seen to be traversed by a dark band contiguous to the moon's limb. No such appearance was visible in this instance. The circumstances of the reappearance of Saturn at the moon's bright limb on May 8, were very similar to those of the reappearance of Jupiter, excepting that there was no depression of the limb where Saturn reappeared such as that which was noticed at the place of Jupiter's reappearance. The comparison of the two occultations seems, therefore, to indicate that the phænomenon seen in the case of Jupiter was in some way connected with the indented form of the moon's limb.

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

October 31, 1859.

A communication was made by Mr. Hopkins "On the construction of a new Calorimeter for determining the Radiating Power of the Surfaces of Heated Bodies."

November 14, 1859.

A communication was made by the Master of Trinity College "On the Mathematical part of Plato's Meno."

November 28, 1859.

The Rev. Dr. Donaldson read a paper "On the Origin and proper value of the word 'Argument.'"

The author first investigated the etymology and meaning of the Latin verb *arguo*, and its participle *argutus*. He showed that *arguo* was a corruption of *argruo* = *ad gruo*; that *gruo* (in *argruo*, *ingruo*, *congruo*) ought to be compared with κρούω, which means "to dash one thing against another," especially for the purpose of making a shrill, ringing noise; that *arguo* means "to knock something for the purpose of making it ring, or testing its soundness," hence "to test, examine, and prove anything;" and that *argutus* signifies "made to ring," hence "making a distinct, shrill noise," or "tested and put to the proof." Accordingly *argumentum* means *id quod arguit*, "that which makes a substance ring, which sounds, examines, tests, and proves it."

It was then shown that these meanings were not only borne out by the classical usage of the word, but also by the technical application of "argument" as a logical term. For it is not equivalent to "argumentation," or the process of reasoning; it does not even denote a complete syllogism; though Dr. Whately and some other

writers on logic have fallen into this vague use of the word, and though it was so understood in the disputations of the Cambridge schools. The proper use of the word "argument" in logic is to denote "the middle term," *i. e.* "the term used for proof." In a sense similar to this the word is employed by mathematicians; and there can be no doubt that the oldest and best logicians confine the word to this, which is still its most common signification.

The author entered at some length into the Aristotelian definition of the *enthymeme*, which may be rendered approximately by the word "argument." He also explained how the words "topic" and "argument" came to denote the subject of a discourse or even of a picture. He showed, by a collection of examples from the best English poets, that the established meanings of the word "argument" are reducible to three: (1) a proof or means of proving; (2) a process of reasoning or controversy made up of such proofs; (3) the subject matter of any discourse, writing, or picture. And he maintained that the second of these meanings ought to be excluded from scientific language.

December 12, 1859.

The following paper from the Astronomer Royal was read:—"Supplement to the proof of the Theorem that 'Every Algebraic Equation has a Root.'"

The author expressed his want of confidence in every result obtained by the use of imaginary symbols, and in this supplement demonstrated that the left-hand member of every algebraic equation of the form $\phi(x)=0$ admitted of resolution, either into real linear factors, or into real quadratic factors.

Professor Miller also made a communication "On a new portable form of Heliotrope, and on the employment of Camera Lucida prisms and right-angled prisms in surveying."

February 13, 1860.

The Rev. H. A. J. Munro read a paper "On the Metre of an Inscription copied by Mr. Blakesley, and printed by him in his 'Four Months in Algeria,' p. 285."

February 27, 1860.

The Rev. Professor Sedgwick made the following communications:—

1. "An account of Mr. Barrett's progress in the Survey of Jamaica, with some remarks on the Distribution of Gold Veins."

2 "Some account of the Geological Discoveries in the Arctic Regions."

March 12, 1860.

The Rev. Professor Challis made a communication "On the Planet within the orbit of Mercury, discovered by M. Lescarbault."

By a recent comparison of the theory of Mercury's orbit with observation, M. Leverrier found that the calculated secular motion of the perihelion of that planet requires to be increased by 38″, and that this difference between observation and theory cannot be accounted for by the attractions of known bodies of the solar system. In a letter addressed to M. Faye, and published in the Paris Meteorological Bulletins of October 4, 5, and 6, 1859, he suggested that the difference might be due to the attraction of a group of small planets circulating between Mercury and the Sun. On December 22 of the same year, M. Lescarbault, a physician and amateur astronomer, residing at Orgères, about sixty miles south-west of Paris, announced in a letter to M. Leverrier that he had seen on March 26, 1859, a small round spot traversing the sun's disk, which he considered to be a planet inferior to Mercury. Naturally much interested by this information, M. Leverrier went to Orgères on December 31, and after closely interrogating M. Lescarbault respecting the particulars of the observation, and the instrumental means by which it was made, he returned with the conviction that the observation was trustworthy, and that a new planet had been discovered (*Comptes Rendus*, January 2, 1860, p. 40).

M. Lescarbault had long conceived the idea of detecting inferior planets by watching the sun's disk for transits, and in 1858 he put his project into execution. He was in possession of a good telescope of $3\frac{3}{4}$ inches aperture and 5 feet focal length, mounted with an altitude and azimuth movement, and provided with a finder magnifying 6 times. The power of the eyepiece employed in the observations of March 26 was 150. Not being furnished with a position-circle, he adopted the following means of obtaining angular measurements. The eyepiece of the telescope and the eyepiece of the finder each had at its focus two wires crossing at right angles, and the wires of the latter were so adjusted that a star seen at their intersection was seen at the same time at the intersection of the wires of the telescope. There were also in the eyepiece of the finder two wires parallel to, and on opposite sides of, each cross-wire, and distant by about 16′. A circular card about 6 inches in diameter, and graduated to half degrees, was placed concentric with the tube of the eyepiece of the finder, and apparently could be moved both about the tube and, with the tube, about the axis of the finder. A cross-wire of the telescope and a cross-wire of the finder were adjusted vertically by looking at a distant plumb-line, and the diameter of the card containing the zero of its graduation was placed vertically by means of a small plumb-line and eye-hole approximately arranged for that purpose. The

mode of using this apparatus for angular measurements will be seen by the following account of the observations. The observer had also a small transit-instrument by which he obtained true time, using for timepiece his watch, which, as it only indicated minutes, required the supplement of a temporary seconds' pendulum.

In the account which M. Lescarbault gives of his observations, he says that it had been his practice to examine with the telescope the contour of the sun for a considerable interval on each day in which he had leisure, and that at length, on March 26, 1859, he saw a small round spot near the limb, which he immediately brought to the intersection of the wires of the telescope. Then, according to his statement, he quickly turned the graduated card till *two* of the wires of the finder were tangents to the sun's limbs, or equidistant from them. But it is evident that to effect an angular measurement in this way, *one* of the middle wires of the finder must have been placed tangentially to the sun's limb at the point of their intersection, to which point the spot had just been brought. Assuming that this operation was performed, the angular distance of the point from the vertical diameter of the sun might be read off, as the account states that it was, by applying the plumb-line apparatus to the graduated card. This method could only give a rough measure of the angular position of a point very near the sun's limb; and in fact M. Lescarbault does not appear to have attempted to determine the position of the spot during the interval between the beginning and the end of the transit. He states that the spot had entered a little way on the sun when he first saw it, and that the time and place of entrance were inferred by estimation.

The following are the immediate results of the observations:—The spot entered at $4^h\ 5^m\ 36^s$ mean time of Orgères at the angular distance of $57°\ 22'$ from the north point towards the west, and departed at $5^h\ 22^m\ 44^s$, at $85°\ 45'$ from the south point towards the west, occupying consequently in its transit $1^h\ 17^m\ 8^s$. The length of the chord it described was $9'\ 14''$, and its least distance from the sun's centre $15'\ 22''$. M. Lescarbault also states that he judged the apparent diameter of the spot to be at most one-fourth of that of Mercury, when seen by him with the same telescope and magnifying power during its transit across the sun on May 8, 1845. The latitude of Orgères is $48°\ 8'\ 55''$, and longitude west of Paris, $2^m\ 35^s$.

From these data M. Leverrier ascertained, by calculating on the hypothesis of a circular orbit, that the longitude of the ascending node is $12°\ 59'$, the inclination $12°\ 10'$, the mean distance 0·1427, that of the earth being unity, and the periodic time 19·7 days. Also he found that the greatest elongation of the body from the sun is 8°, the inclination of its orbit to that of Mercury 7°, the real ratio of its diameter to Mercury's 1 to 2·58, and that its volume is one-seventeenth the volume of Mercury on the supposition of equal densities. This mass is much too small to account for the perturbation of Mercury's perihelion. According to these results, the periods at which transits may be expected are eight days before and after April 2 and October 5, the body being between the earth and sun

near its descending node at the former period, and near its ascending node at the latter.

After the announcement of this singular discovery, it was found that other observations of a like kind had been previously made. Several instances are collected by Professor Wolf in the tenth number of his *Mittheilungen über die Sonnenflecken*, eight of which are quoted in vol. xx. (p. 100) of the Monthly Notices of the Royal Astronomical Society. Two of these, the observation of Stark on October 9, 1819, and that of Jenitsch on October 10, 1802, agree sufficiently well with the calculated position of the node of the object seen by Lescarbault. But the spot seen by Stark is stated to have been about the size of Mercury.

Capel Lofft saw at Ipswich, on January 6, 1818, at 11 A.M., a spot of a 'sub-elliptic form,' which advanced rapidly on the sun's disk, and was not visible in the evening of the same day (Monthly Magazine, 1818, part 1, p. 102).

Mr. Benjamin Scott, Chamberlain of London, saw about midsummer of 1847 a large and well-defined round spot, comparable in apparent size with Venus, which had departed at sunrise of the next day (Evening Mail, January 11, 1860).

Pastorff of Buchholz records that he saw on October 28 and November 1, 1836, and on February 17, 1837, *two* round black spots of unequal size, moving across the sun at the respective hourly rates of 14′, 7″, and 28′. Also he announced, January 9, 1835, to the Editor of the *Astronomische Nachrichten*, that "six times in the previous year he had seen two new bodies pass before the sun in different directions and with different velocities. The larger was about 3″ in diameter, and the smaller from 1″ to 1″·25. Both appeared perfectly round. Sometimes the smaller preceded, and at other times the larger. The greatest observed interval between them was 1′ 16″: at times they were very near each other. Their passage occupied a few hours. Both appeared as black as Mercury on the sun, and had a sharp round form, which, however, especially in the smaller, was difficult to distinguish." Schumacher considered it his duty as editor to insert the communication, but evidently did not give credit to it (*Astron. Nachr.* No. 273).

In vol. ii. of the Correspondence between Olbers and Bessel, mention is made in p. 162 of an observation at Vienna by Steinhübel, of a dark and well-defined spot of circular form which passed over the sun's diameter in five hours. Olbers, from these data, estimates the distance from the sun to be 0·19, and the periodic time thirty days. It is remarkable that Stark saw about noon *of the same day* a singular and well-defined circular spot, which was not visible in the evening. This is one of the instances in vol. xx. of the Monthly Notices of the Astronomical Society.

These accounts appear to prove that transits of dark round objects across the sun are real phænomena; but it would perhaps be premature to conclude that they are planetary bodies. If the object observed by Lescarbault be a planet, it is certainly very surprising that it has not been often seen. Schwabe, after observations of the sun's

face continued through thirty-three years, has recorded no instance of such a transit. It is probable that now attention has been especially drawn to the subject, future observations, accompanied by measures (of which Lescarbault's are the first instance), may throw light on the nature of these phænomena.

April 23, 1860.

Professor De Morgan read a paper "On the Syllogism, No. IV., and on the Logic of Relations."

In the third paper were presented the elements of a system in which only *onymatic* relations were considered; that is, relations which arise out of the mere notion of nomenclature—relations of name to name, as names. The present paper considers relation in general. It would hardly be possible to abstract the part of it which relates to relation itself, or to the author's controversy with the logicians, who declare all relations *material* except those which are onymatic, to which alone they give the name of *formal*. Mr. De Morgan denies that there is any purely formal proposition except "there is the probability *a* that X is in the relation L to Y;" and he maintains that the notion 'material' *non suscipit magis et minus*; so that the relating copula is as much materialized when for L we read *identical* as when for L we read *grandfather*.

Let X . . LY signify that X stands in the relation L to Y; and X . LY that it does not. Let LM signify the relation compounded of L and M, so that X . . LMY signifies that X is an L of an M of Y. In the doctrine of syllogism, it is necessary to take account of combinations involving a sign of *inherent quantity*, as follows:—

By X . . LM′Y is signified that X is an L of *every* M of Y.

By X . . L,MY it is signified that X is an L of *none but* Ms of Y.

The *contrary* relation of L, not -L, is signified by *l*. Thus X . LY is identical with X . . *l*Y. The converse of L is signified by L^{-1}: thus X . . LY is identical with Y . . L^{-1}X. This is denominated the L-*verse* of X, and may be written LX by those who prefer to avoid the mathematical symbol.

The attachment of the sign of inherent quantity to the symbols of relation is the removal of a difficulty which, so long as it lasted, prevented any satisfactory treatment of the syllogism. There is nothing more in X . . LM′Y than in every M of Y is an L^{-1} of X, or MY))L^{-1}X, X and Y being individuals; and nothing more in X . . L,MY than in L^{-1}X))MY, except only the attachment of the idea of quantity to the combination of the relation.

When X is related to Y and Y to Z, a relation of X to Z follows: and the relation of X to Z is compounded of the relations of X to Y and Y to Z. And this is syllogism. Accordingly every syllogism has its inference really formed in the first figure, with both premises affirmative. For example, Y . LX and Y . . MZ are premises stated

in the third figure: they amount to $X \,.\,.\, L^{-1} Y$ and $Y \,.\,.\, MZ$, giving $X \,.\,.\, l^{-1} MZ$ for conclusion. This affirmative form of conclusion may be replaced by either of the negative forms $X \,.\, L^{-1} M'Z$ or $X \,.\,.\, l_{,}^{-1} mZ$.

The arrangement of all the forms of syllogism, the discussion of points connected with the forms of conclusion, the extension from individual terms in relation to quantified propositions, the treatment of the particular cases in which relations are convertible, or transitive, or both—form the bulk of the paper, so far as it is not controversially directed against those who contend for the confinement of the syllogism to what Mr. De Morgan calls the *onymatic* form.

An appendix follows the paper, on syllogism of transposed quantity, in which the number of instances included in one premise is equal to the whole number of existing instances of the concluding term in the other premise.

Mr. J. H. Röhrs also read a paper "On the Motion of Bows, and thin Elastic Rods."

May 7, 1860.

The Rev. Professor Sedgwick made a communication "On the Succession of Organic Forms during long geological periods; and on certain Theories which profess to account for the origin of new species."

May 21, 1860.

The Public Orator read a paper "On the Pronunciation of the Ancient and Modern Greek Languages."

He gave a rapid sketch of the "Reuchlin and Erasmus" controversy in the sixteenth century, especially the part taken in it at Cambridge by Cheke, Smith, Ascham, and Bishop Gardiner; and then proceeded to show how the proper sounds of the Greek letters may be determined from the following sources:—

1. Distinct statements of grammarians.
2. Incidental notices in other ancient authors.
3. Variations in writing of inscriptions and MSS.
4. Phonetic spelling of cries of animals.
5. Puns and riddles.
6. The value of the respective letters in other languages employing the same alphabet, especially Latin.
7. The way in which Latin proper names are spelt in Greek, and *vice versâ*.
8. The traditions of pronunciation preserved in modern Greek.

He concluded that, on the whole, the method of Erasmus approached more nearly to the ancient pronunciation than that of Reuchlin.

"But," he proceeded, "when we consider the untrustworthiness of each of these sources of evidence taken singly, and when moreover we find them often in conflict with one another, it cannot be expected that the result should be very certain or very satisfactory. There are also other considerations which enhance the difficulty of the inquiry. As there were very marked dialectic varieties in Greece, so there may have been local variations even in Attica itself.

"The pronunciation, too, changed from time to time. Plato gives us proof of this in the 'Cratylus.'"

After quoting several instances, and showing that great changes both in pronunciation and spelling had taken place in modern languages, French, Spanish, and English, "it would," he said, "be hopeless to attempt to determine the pronunciation of any language by a reference to its orthography at a time when both were perpetually changing. But in the history of every nation there arrives a time when the creative energy of its literature seems to have spent itself; when, instead of developing new forms, men begin to look back and not forward, to comment and to criticise. Then it is that a language begins to assume, even in minor and merely outward points, such as pronunciation and spelling, a fixity and rigidity which it retains with scarcely any change so long as the nation holds together. Such a period in Greek history was that which began with the grammarian sophists in the fifth century B.C., and culminated in Aristarchus and Aristophanes of Byzantium. In the spelling and pronunciation of Greek there was probably very little change from that time to the end of the third century A.D."

October 19, 1860.

Dr. Paget made a communication "On some Points in the Physiology of Laughter."

November 12, 1860.

The Public Orator read a paper (a sequel to that on May 21) "On the Accentuation of Ancient Greek."

The question of accents was not discussed in the Reuchlin and Erasmus dispute. At that time all pronounced according to the system of accents introduced by the Greeks of Constantinople, who first taught the ancient language to the Italians.

It was probably in Elizabeth's reign that we began to disuse the old pronunciation of vowels both in Greek and Latin; and concurrently with this change we, as well as the other nations of Europe, began to pronounce Greek, not with the modern Greek, but with the Latin accent. The reasons were:—

1. Teachers speaking the modern Greek were no longer required, so the tradition was not kept up.

2. It saved much trouble to pronounce both languages with the same accentuation.

3. The Greek accent perpetually clashes with quantity; the Latin much more rarely; never, indeed, in that syllable of which the quantity is most marked—the penultima.

Isaac Vossius (1650–60) advocated the disuse of accentual marks altogether, as the invention of a barbarous age to perpetuate a barbarous pronunciation.

After showing the meaning of the word 'accent' as applied to modern languages, and discussing the accentuation of the German, English, French, &c., he proceeded to say:

"There are three methods of emphasizing a syllable:—

1. By raising the note;
2. By prolonging the sound;
3. By increasing its volume.

"Scaliger, *De Causis Linguæ Latinæ*, lib. ii. cap. 52, recognizes this division when he says that a syllable may be considered of three dimensions in sound, having height, length, and breadth.

"Now in our own language, when we accent a syllable, which of these dimensions do we increase? Generally all three, but not necessarily; for when the prayers, for example, are intoned, *i. e.* read upon one note, the accent is marked by increasing the volume of sound (the third method), which involves also a longer time in utterance, *i. e.* a lengthening of quantity. In speaking, all three methods are employed, but one more prominently than the other, according to individual peculiarities of the speakers. What we blend, the Greeks kept distinct.

"We cannot understand the Greek system unless we bear this in mind. They never confounded accent with quantity. Ineradicable habit prevents us from reverting in practice to their method, just as they would have been unable to comprehend ours.

"It is clear from Dionysius, *De Comp. Verb.* lib. xi. cap. 75, that the dialogue in tragedy preserved the ordinary accentuation, which was disregarded only in choral passages set to music."

The practical conclusion was this: that while it would be desirable, if possible, to return to the Erasmian system of pronunciation, it would be extremely absurd to adopt the barbarous accentuation of modern Greek, which has quite lost the old essential distinction between accent and quantity. In this respect, as we cannot recover practically the ancient method, it is better to keep to our own system of the Latin accent, which does not confuse the learner's notion of quantity in verse as the modern Greek does.

An Athenian boy has the greatest difficulty in comprehending the rhythm of Homer or Sophocles. Hence it is not blind prejudice (as Professor Blackie asserts) which makes us keep to our old usage, but a well-grounded conviction that we should lose more by changing than we should gain.

November 26, 1860.

Professor Challis made a communication "On the Solar Eclipse of July 18, 1860."

December 10, 1860.

Mr. Seeley read a "Notice of Opinions on the Red Limestone at Hunstanton."

Professor Miller also described "An Instrument for measuring the radii of arcs of Rainbows."

February 11, 1861.

Mr. H. D. Macleod read a paper "On the present State of the Science of Political Economy."

The writer took a general survey of the science as it at present exists, testing several generally received doctrines by the principles of inductive logic, and earnestly enforcing the necessity of a thorough reform of the whole science, which must be constructed on principles analogous to those of the other inductive sciences.

February 25, 1861.

Dr. Humphry made a communication "On the Growth of Bones."

March 11, 1861.

The Master of Trinity made a communication "On the Timæus of Plato."

PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

1843–65.

VOL. I PART 16

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PROCEEDINGS

OF THE

CAMBRIDGE PHILOSOPHICAL SOCIETY.

October 26, 1863.

A communication was made by Mr. H. D. Macleod "On the Theory of Banking."

November 9, 1863.

Communications were made by Dr. Humphry, "The Results of Experiments on the Growth of the Jaw."

By Mr. Todhunter "On a Question in the Theory of Probabilities."

November 23, 1863.

A communication was made by Professor Challis "On the Meteor of August 10, 1863."

December 7, 1863.

A communication was made by Dr. Akin "On the Origin of Electricity."

February 8, 1864.

A communication was made by Professor Liveing "On the new Metal, Thallium."

February 22, 1864.

Communications were made by Professor Cayley "On a Case of the Involution of Cubic Curves," and "On the Theory of Involution."

By Mr. Spencer "On Vitality."

March 7, 1864.

Communications were made by Mr. Harry Seeley—

1. "On the Significance of the Succession of Rocks and Fossils."

2. "On Pterodactyles, and on a new Species of *Pterodactylus machærorhynchus*."

April 18, 1864.

Communications were made by Mr. Röhrs "On the Strains and Vibrations of Ordnance under the Action of Gunpowder."

By Professor Cayley "On the Classification of Cubic Curves," and "On Cubic Cones and Curves."

May 2, 1864.

Communications were made by Mr. Harry Seeley—

1. "Note on *Palæobalæna Sedgwicki* (nob.), a Whale from the Oolite."

2. "On the Fossil Birds of the Upper Greensand, *Palæocolyntus Barretti* (nob.) and *Pelargonis Sedgwicki* (nob.)."

3. "On the Osteology and Classification of Pterodactyles, Part II., with descriptions of the new species *P. Hopkinsi* and *P. Oweni*."

May 16, 1864.

Papers were read by Professor De Morgan—

1. "A Theorem relating to Neutral Series."

The theorem is as follows. If $a_0 - a_1 + a_2 - \ldots$ be a convergent series which has the limiting form $1 - 1 + 1 - \ldots$, and if a_z be of

continuous law, so that $a_{z+1} : a_z$ finally approaches towards a limit, the limit towards which the series approaches as its form approaches neutrality is $a_z - a_{z+1}$ divided by $a_z - a_{z+2}$. And this limit is always $\frac{1}{2}$. If there be in the series a cycle of laws involving an even number of terms, so that

$$a_{2nz} - a_{2nz+1},\ a_{2nz+1} - a_{2nz+2},\ \ldots\ a_{2nz+2n-1} - a_{2nz+2n}$$

approach in ratio to $k_0, k_1, \ldots k_{2n-1}$, then the two series $a_0 - a_1 + a_2 - \ldots$ and $a_1 - a_2 + a_3 - \ldots$, which have unity for their sum, have the ratio of $k_0 + k_2 + \ldots + k_{2n-2}$ to $k_1 + k_3 + \ldots + k_{2n-1}$. But if the cycle have an odd number of terms, each of these series is $\frac{1}{2}$, just as if the law had been continuous. The demonstration is founded upon the following theorem :—If $P_0 + P_1 + \ldots$ and $Q_0 + Q_1 + \ldots$ be diverging series, whether of increasing or decreasing terms, their two infinite sums are in the final ratio of P_z to Q_z. Applications of this theorem are given to the determination of a large number of terms of $1^n + 2^n + \ldots$ when n is -1 or greater, and to the determination of the usual approximation to $1 . 2 . 3 \ldots n$ when n is great.

2. "On Infinity, and on the Sign of Equality."

The author professes himself satisfied of the *subjective* reality of the notions of infinitely great and infinitely small. His paper, as far as it deals with various objections by various modes of answer, is not capable of abstract; but four points, on which he especially relies, may be stated as follows :—

1. The concepts of the mind are divisible into imageable and unimageable : the first can be pictured and placed before the mind's eye; the second cannot. The mathematician, dealing in great part with imaged concepts, is apt to repel the unimageable, as if it could not be a legitimate object of mathematical reasoning. But all that is necessary to reasoning is knowledge of the connexion of subjects and predicates. Infinite quantity is unimageable in its relation to finite quantity, but not therefore inconceivable, nor destitute of known attributes. A million of cubic miles is as destitute of image as infinite space; nevertheless it is a conception the attributes of which give known propositions.

2. Number, or enumeration as distinguished from multitude, is a concept from which no notion of infinity can be gained; but much perplexity has arisen from the attempt to make it a teacher of this subject. Abstract number has more than one affection which is derived from the concrete in such manner that the two abstractions. number and its affection, cannot have their function explained excepting by return to the concrete. Such affections are the divisible unit, on which the doctrine of fractions is founded, and the opposition of positive and negative. The representation of infinite and of infinitesimal number is a third affection of the numerical, which cannot be explained on purely numerical notions.

3. The infinite is not a kind of terminus to the finite, but another *status* of magnitude, such that no finite, however great, is anything but an infinitesimal of the infinite. And the same may be said of each order of infinity with reference to the one below it.

4. The symbol $\frac{1}{0}$, the infinite of common algebra, represents an extreme of infinite which can no more be attained by passage through orders of infinity, than any infinite by passage from finite to finite.

Each of these positions comes into conflict with some of the usual arguments for or against the introduction of infinites.

The second part of the paper is on the meaning of the sign of equality. Mr. De Morgan contends for the ultimate attainment of a purely formal algebra, in which every transformation shall have meaning and validity in every possible case. He points out certain difficulties and inconsistencies in the ordinary use of the sign of equality, which can, he affirms, receive a consistent explanation on the extension which he proposes, and which, to some extent, he considers as virtually adopted.

His notion is that *equality*, strictly so called, is but a species of the genus *undistinguishable*; and that the actual use of the sign (=) shows a leaning to the generic definition. Every order of infinites or infinitesimals has its own metre, and the sign (=) indicates undistinguishability with reference to the metre, which is often in thought, but for which no symbol is employed. Algebraical changes may or may not demand or permit changes in the metre. It would be impossible to give any further account, with justice to the subject, in a short abstract.

3. By Mr. Harry Seeley "On Saurornia, and the Classification of Pterodactyles, Part III."

October 31, 1864.

Papers were read by Professor Selwyn "On Autographs of the Sun."

By A. R. Catton, B.A., St. John's College, "On the Constitution of Chemical Compounds."

November 14, 1864.

A communication was made by G. F. Browne, M.A., St. Catharine's College, "On certain Ice-Caverns."

November 28, 1864.

Papers were read by Dr. Humphry on the question, "Is the Vertebral Theory of the Skull to be abandoned?"

This communication was intended partly as a reply to the opinion expressed by Professor Huxley in his lectures on comparative anatomy, that the vertebral hypothesis of the skull has been abolished by the recent discoveries in development. Dr. Humphry commencd by calling attention to the Laws of Uniformity of Plan, and Variety in Detail, which prevail throughout the animal kingdom, and, indeed, throughout the material system, and which the recent discoveries

by the microscope have shown to rule over the ultimate structure and formation of all the tissues of the body. The discovery of the illustration of these laws in the plan of cell-formation of the tissues, and in the development of all animal and vegetable structure from the simple cell-form, he regarded as the grandest discovery in physical science that has taken place in our time. Of late years, the attention of anatomists has been much directed to the exemplification of these laws in the vertebrate classes, to tracing the uniformity of plan, especially in the skeleton, through the variety in detail which the members of these classes exhibit. This constitutes the branch of anatomy called "Homology." The general features of the plan upon which vertebrate animals are constructed are clear enough in all of them. Osseous segments, or vertebræ, with neural and visceral processes, enclosing respectively the neural and visceral centres, constitute the trunk, including neck, chest, loins, &c. Probability is in favour of the view propounded by Goethe and Oken, and worked out by Oken and Owen, that the skull falls in with the Law of Uniformity, and corresponds with the rest of the frame in having a vertebral composition. It is by all anatomists admitted to be segmentally constructed. Most anatomists are agreed as to the number of segments. Ought not, therefore, these segments to be described by the same name as those of which they form a continuation, especially as they bear the same relations to the neural and visceral centres, and the same or nearly similar relations to the nerves and blood-vessel? In their mode of development, too, the segments of the skull show a marked general correspondence with those of the trunk. The chorda dorsalis, around which the vertebral centres are formed, extends at any rate halfway along the base of the skull; and the bodies and arches of the cranial segments are evolved from a continuation of the same embryonic structure (the "vertebral plates") as the trunk segments—the chief difference being that in the trunk segmentation takes place at an earlier period than in the head. In the trunk, it is observed in the vertebral plates; and these primitive segments are called "protovertebræ." They appear not to exist in the head. The segmentation, however, takes place in the cranium as soon as ossification begins, even if it does not do so before; and the significance of the protovertebræ as distinctive features between the skull and the trunk is diminished, first, by their being related to the formation of the nerves as much or more than to that of the vertebræ; and secondly, by their not really corresponding with the vertebræ, each permanent vertebra being formed by a half of two protovertebræ. Dr. Humphry expatiated on this and other points in the development of the skull, and expressed his decided opinion that the differences between the development of it and of the trunk vertebræ were by no means sufficient to controvert the view—which coincides with the Law of Uniformity, and which is confirmed by the segmental construction of the skull, by the relation of its components to surrounding parts, and by so many fundamental resemblances in development—that the same name may be applied to the segments of the skull and of the trunk,

and that the one, as well as the other, consists of vertebræ modified to meet the requirements of the parts in which they are found. He concluded by stating that the greater number of those anatomists to whose observations we are indebted for most of our knowledge of the development of the skull and of the trunk, are agreed that the differences between the mode of formation of the segments in the two form no real argument against the vertebral character of either; and he thought stronger reasons must be adduced than had yet been shown before the anatomists could be called upon to abandon the vertebral theory of the skull.

By Professor De Morgan "On the Early History of the Signs + and —."

An account is given of the work on arithmetic of John Widman, printed in 1489, in which the signs + and — are used to denote *more* and *less*. The use made is twofold: $a+b$ signifying that b more than a is wanted, infers a direction to add b to a. But $a+b$ in the old rule of *false position* is used to signify that the assumption of a for the answer gives b too much in the solution. This last usage was continued by many writers through the greater part of the sixteenth century.

Some account is given of the *Die Coss* of Chr. Rudolf, which passes for the first work in which + and — are used. The first edition of his work being lost, a question is raised as to how far the second edition, edited by Stifel, is a fair reprint of the first. A Latin translation of this first edition is said to be in the Imperial Library at Paris.

From the manner in which Widman introduces his signs, Mr. De Morgan thinks there is some ground to suspect that they were originally *warehouse marks*, indicating the scale into which smaller weights were introduced to make the balance, when the nearest number of larger weights had been put in. This point and others require the examination of older works, print and manuscript.

February 13, 1865.

Communications were made by Professor Cayley "On Abstract Geometry."

By Professor Clifton, "Note on the Early History of the Signs + and —."

February 27, 1865.

Mr. Alfred Newton, M.A., F.L.S., communicated some "Notes on Spitzbergen," of which the following is an abstract.

The author stated that last summer he accompanied Mr. Edward Birkbeck on a voyage to Spitzbergen, in that gentleman's yacht, the 'Sultana,' R.S.Y.C. After giving a slight sketch of some of the principal voyages which had been made to that country, he pro-

ceeded to say that the 'Sultana' left Hammerfest on the 3rd of July, in company with a Norwegian sloop which had been specially fitted to encounter the ice, and chartered to attend upon the yacht. On the 6th they arrived at the entrance of Stor Fjord, which was found to be entirely blocked by the ice. Horn Sound and Bell Sound were subsequently discovered to be in the same condition. They then made for Ice Sound, and anchored in Safe Haven on the 9th. Ice Sound was described as a very much larger inlet than it is represented in the charts to be, extending at least fifty miles into the interior. All the valleys on the north side, and consequently having a southern aspect, are completely occupied by large glaciers, which, with one exception, are only terminated by the sea. The single exception consists of a small but remarkable glacier suspended on a hill-side, some 360 yards from the beach, resting conformably on its own moraine, and having no apparent means of discharge. The author supposes this last was effected by filtration through its bed. The south side of Ice Sound contains several bays of considerable size; and the valleys opening upon it, and therefore having a northern aspect, were entirely free from glaciers; the observation being directly opposed to the account given of the Spitzbergen glaciers by Sir John Richardson in his 'Polar Regions.' A great many reindeer frequent this part of the country, and countless numbers of sea-fowl breed on such of the high cliffs around the Sound as are inaccessible to the Arctic foxes. In Ice Sound Mr. Birkbeck's party had the pleasure of meeting the Swedish Scientific Expedition under Professor Nordenskjold, who are engaged in measuring an arc of the meridian. On the 4th of August the party separated, some going to the eastward in the Norwegian sloop, while the yacht made another ineffectual attempt to ascend the Stor Fjord. The sloop sailed as far as Ryklis Islands, but was stopped by the ice. She then proceeded further east in the hope of getting round the pack, and came in sight of "Commander Gile's land," the existence of which had been so long doubted, it having been ignored by Sir John Richardson in his work, and in the Admiralty Chart of Spitzbergen. It appears to lie about sixty miles east of the entrance to Walter Thymen's Strait, and its flat or round-topped hills (so different from those of Spitzbergen) were very plainly seen. The author stated that in 1859 the master of a Norwegian vessel landed upon it, and he produced a pebble which had then been brought thence, in proof of the reality of its existence as land, and not either ice or fog-bank. He then proceeded to remark on the driftwood with which the shores of the "Thousand Islands" are strewn, which he believed to be certainly of Siberian origin, and not brought, as sometimes imagined, by the Gulf-stream,—stating that though often worm-eaten, he had never observed any signs of barnacles upon it. He then commented on the discovery of the passage from the top of Stor Fjord to Hinlopen Strait, of which there had long been a traditionary knowledge, though it was not effected till 1859; and showed, from the Swedish surveys in 1861, that this passage must lie some thirty miles further south than the position assigned to it on the Admiralty charts, thus

affording another instance of our imperfect knowledge of the geography of Spitzbergen. In conclusion, the author stated that he left Spitzbergen on the 21st of August, the sun having set a night or two previously for the first time, and the salt water begun to freeze; and he warmly urged his audience to support the further circumpolar exploration which has been lately proposed by Captain Sherard Osborne; and said that, as a zoologist, he could declare there were many questions of the very highest interest which could only be solved by a new Arctic expedition.

Professor Cardale Babington and Mr. Harry Seeley made communications respectively on the plants and on the fossils brought by Mr. Newton from Spitzbergen.

March 13, 1865.

A communication was made by Professor Liveing "On Guncotton."

March 27, 1865.

A communication was made by Professor Miller "On the Crystallographic Methods of Grassman, Hessel, Frankenheim, and Uhde, and on their employment in the investigation of the general geometrical properties of Crystals."

May 1, 1865.

Communications were made by Mr. Harry Seeley—

1. "On the Cambridge Greensand.—Part I. The Rock and its Origin."

2. "On the Gravel and Drift of the Fenland.—Part II. Theory."

May 15, 1865.

A communication was made by Professor Churchill Babington "On the Coinage of England before the Norman Conquest."

May 29, 1865.

A paper was read by Mr. Todhunter "On the Method of Least Squares."

The object of this communication is principally to demonstrate a very remarkable result which Laplace enunciated, without demonstration, in the first Supplement to his work on 'Probabilities.' An exposition is also given of the process adopted by Laplace for investigating the method of least squares. Laplace's process is generalized and extended; and results which he obtained for the case of two elements are shown to hold for any number of elements. The mathematical part of the investigation consists chiefly in the evolution of certain definite multiple integrals.

October 30, 1865.

The following officers were elected:—

President	Rev. W. H. Cookson, D.D.
Vice-Presidents.	Mr. I. Todhunter. Dr. Paget. Professor Challis.
Treasurer	Rev. W. M. Campion.
Secretaries ..	Professor Cardale Babington. Professor Liveing. Rev. T. G. Bonney.
New Members the Council.	Professor Selwyn. Rev. W. G. Clark. Mr. R. Potter. Rev. N. M. Ferrers.

The following communications were made to the Society:—

By Mr. A. R. Catton—

1. "On the Synthesis of Formic Acid."

2. "On the possibility of accounting for the double refraction of Light by the vibrations of a *continuous* elastic medium kept in a state of constraint by the action of the material molecules."

By Professor Cayley—

3. "A new Theorem on the Equilibrium of four Forces acting on a solid Body."

Defining the "moment of two lines" as the product of the shortest distance of the two lines into the sine of their inclination, then, if four forces acting along the lines 1, 2, 3, 4 respectively are in equilibrium, the lines must, as is known (Möbius), be four generating lines of an hyperboloid; and if 12 denote the moment of the lines 1 and 2, and similarly 13 the moment of the lines 1 and 3, &c., the forces are as

$$\sqrt{23.34.42}:\sqrt{34.41.13}:\sqrt{41.12.24}:\sqrt{12.23.31}.$$

Calling the four forces P_1, P_2, P_3, P_4, it follows as a corollary that we have

$$P_1P_2.12=12.34\sqrt{13.42}.\sqrt{14.23}=P_3P_4.34;$$

viz. the product of any two of the forces into the moment of the lines along which they act is equal to the product of the other two forces into the moment of the lines along which they act,—which is equivalent to Chasles's theorem, that, representing a force by a finite line of proportional magnitude, then in whatever way a system of forces is resolved into two forces, the volume of the tetrahedron formed by joining the extremities of the two representative lines is constant.

November 13, 1865.

By Professor Sedgwick, F.R.S., "A Sketch of the Geology of the Valley of Dent, with some account of a destructive Avalanche which fell in the year 1752."

The valley of Dent lies in the north-west corner of Yorkshire, which is thrust in between Westmoreland and Lancashire, beyond the natural limits of the county. The upper part of the valley is excavated in the carboniferous groups which are continued southwards into Nottinghamshire, and northwards into Durham and Northumberland, and through the greatest part of their range form the watershed between the east and west coasts of England. All the valleys that drain down to the Lune are partly formed in rocks of the carboniferous age. In the upper part of Dent Dale, which is one of these tributaries, the great scar-limestone appears only near the bottom of the valley, while the sides are formed of soft shale alternating with harder bands of sandstone and limestone; and the whole series is capped by mill-stone grit. The rainfall in some portions of the Lake mountains is not less than 150 or 160 inches in the year. Among the neighbouring carboniferous mountains the rainfall is much less; but still it is at least three times the English average; and the winter fall of snow is in some years enormous. Hence the *becks*, or mountain-streams, are often greatly swollen, and the *gills*, or lateral branches, frequently descend in brawling torrents from the mountain-side into the lower valley through deep ravines and lateral valleys that have been excavated out of the shales and sandstones in the course of past ages. On rare occasions a great fall of snow, accompanied by a violent wind, will almost fill up the ravines and lateral valleys, and form a dam across the descending water; and should there be a sudden thaw afterwards, the descending gills may be held up for a while till the pressure of the water drives down the barrier, and an avalanche is formed of mingled snow and water (provincially called *a brack*), which rushes down with the roar of thunder, and bears all before it into the *beck* below. On the 6th of February, 1752, a very large one fell, destroying several houses and farm buildings, and killing seven people, besides several head of cattle. The following letter, written by an eye-witness, describes the catastrophe (the spelling and punctuation have been slightly modernized):—

"Harbourgill, 6th of the 2nd month, 1752.

"Dear Bro' and Sister,—

"These few lines I hope will find excuse: for it's not without a cause that I have written no sooner to you. I fully purposed to have seen you a considerable time since: but now, as things are at present, I have lost all hopes of coming. Yet through the good providence of Heaven we are all alive and pretty well in health: which is more than could be well expected, considering what dismal times it has been with us in Dent. I hope I shall never live to see the like again: for we had the greatest storm of wind and snow that conti-

nued for above a week with very little intermission: so that all the watercourses, both in the mountains and elsewhere, were made level; the like never being remembered, for it excited the curiosity of several persons to view them with wonder and astonishment: yet little thinking that the consequences would have been so tragical to many. For at the breaking up of the storm (*i. e.* frost) it began to rain exceedingly in the evening, which continued all night and the next day to that degree that, by 11 o'clock, the dismal scene began. For the snow in the watercouses being no longer able to sustain the great quantities of water, all began to slide down the mountains together with incredible swiftness, driving great rocks, stones, and earth, all before it; roaring like claps of thunder; which made us run out of doors to see what was coming upon us. We ran to look at the Gill; and we directed our sights (by the noise that it made) the right way; and the frightfulness of the appearance at the very first sight, which was when about the middle of the pasture, made us run for our lives; and we got no further than from the yet (*i. e.* gate) to the sycamore trees, before the stable, peat-house, and all the calf-parrack (*i. e.* paddock) and cow-parrack, was in a heap of the most shocking ruins that ever your eyes beheld. I believe from the first sight of it, when it was coming, till all was overturned, was less than the quarter of a minute's time. It has brought rocks down past the middle of the houme, which had gone through the peat-house and stable, that I think three or four yoke of oxen could not be able to move. The poor old horse was crushed to pieces in a moment. Nothing but the good providence of God has preserved us from perishing; for it's amazing to think how the barn stood the violence of the shock. The waters run round our dwelling house, broke down the garden wall, and continued running through it till next day in the morning; so that it's become a bed of sand. It was about 11 o'clock when this happened, and we went from place to place, not knowing where to be safe, expecting every moment more of the like nature; which accordingly happened; for I think in the space of two hours the face of things was so changed that one scarcely could have known them. For they came down almost every slack*, carrying all the walls before them; so that we were obliged to run from one place to another to escape their fury, which was with difficulty: for it continued raining extremely, that we were wet to the naked skin, not daring to come in any house. And it drawing towards night, we resolved to make an attempt to get to brother John's, and accordingly set forwards, and got up at our pasture head on to the moor, and with difficulty got over Harbourgill, and so forwards to the Munkeybeck. But we knew that the bridge was broke down, so that we must be obliged to pass it somewhere on the moor, and we waded through the water and snow till we were almost spent in extreme wet and fatigue; and at last got over a little below where our peat-fell is (tho' with very great hazard of our lives), at last, my poor old Father and Betty being almost quite spent, he having only one

* Slack (*coom* or hollow in the hill-side).

shoe on one foot the greatest part of that time. Then when we were got over, it gave us some fresh encouragements, and we arrived at Bro' Johns just before it was dark, where we were thankful to see the faces of one another in a place of more safety. We went three nights successively to Bro' John's to lodge, not daring to stay about the old place. Old Francis Swinbank [*rect.* Swithinbank] and Thomas Stockdale's whole family perished in a moment about the same time that the thing happened with us, being seven in number. Likewise John Burton, Stone House, had a barn swept away and a cow killed.

"I hope these few broken hints will be excused, for I am not very good at writing at this time, all being so in confusion. Sr. greatly desires you would come to see us as soon as well can. For our love is very much towards you. You perhaps may think I have outstretched, but if you please to come your eyes will convince you to the contrary. For I have not told you one half. So shall conclude your very loving Brother,

"THOMAS THISTLETHWAITE."

"Betty's kind love is to you both, but Sr. in particular."

November 27, 1865.

Mr. J. W. Clark, Trinity College, read a paper upon the Rib of a Whale found by some fishermen near Sherringham (four miles N.W. of Cromer). It was discovered after a high tide, which caused a fall of the cliff; it was reported to have been imbedded in drift gravel. Mr. Clark stated he had compared it with the rib of *Physalus*, with which it did not agree; for the tubercle and head of the rib were very much wider apart in the fossil specimen than in *Physalus*. It resembled *Balænoptera* more nearly, and still more closely *Balæna mysticetus*. It was probably the fourth or fifth rib of the left side. He remarked that a few months since some cervical vertebræ had been found at Plymouth which probably belonged to *Balæna Biscayensis*, a whale which was nearly extirpated in the sixteenth century. It was, however, still occasionally met with, a cow and cub being not long since seen near S. Sebastian, and the latter captured. He could not positively refer the rib exhibited to any known species.

Professor Sedgwick remarked that large cetacean vertebræ had been found at Landbeach, Cambridgeshire, and in the Crag of Norfolk and Suffolk. This was the largest rib that he had ever seen; but he had great doubt, from its general appearance, whether it could have come out of the gravel.

Mr. Newton remarked that our knowledge of whales had of late been greatly enlarged; for of the bodies which had been stranded so many new species and even new forms had been observed, that hardly two had been found to be identical.

Professor Miller exhibited two new forms of heliotrope, explaining at the same time the difficulties in signalling, which they were intended to overcome, commenting upon the relative merits of those invented by Gauss, Steinheil, and others, and explaining the special advantages of the two which he exhibited.

Professor Miller also communicated a Supplement to the Crystallographic method of Grassmann.

Mr. G. F. Browne, St. Catherine's College, communicated some Notes upon some Ice-caves explored during the summer of 1865. Two of these he had visited during the previous summer, and he found that there was a somewhat greater quantity of snow in the caverns than there had formerly been. In the first cave he again examined a pit in the ice about 70 feet deep, but, owing to the dangerous condition of the ice, was unable to descend into it. In the second cave he had again cut through a curtain of ice into an icy tunnel; but this year the diameter of the tunnel was so much smaller that he was unable to descend it, although provided with ropes for the purpose. He described some flies found inside the tunnel. The third cave had not previously been explored; it was an oval in shape, with a level floor of ice. He had descended for about 12 feet between the ice and the rock, and there found a narrow tunnel which appeared to lead to a subglacial reservoir containing water. He ascertained that the ice was at least 24 feet thick; but it was impossible to descend the tunnel.

Mr. Bonney, who had accompanied Mr. Browne, made some observations on the general character of the country, expressing his opinion that the glacières were formed by the accumulation of snow in suitable fissures; and remarking that the prismatic structure of the ice noticed last year by Mr. Browne was very conspicuous.

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